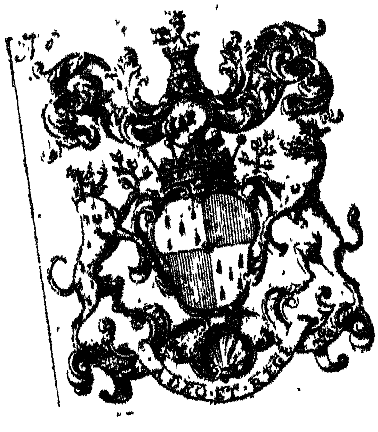

DEO · ET ·

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Conſiderable Parts of the WORLD.

VOL. LXII.

LONDON:

Printed for LOCKYER DAVIS, in *Holbourn*,
Printer to the ROYAL SOCIETY.

M.DCC.LXXII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it had been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the

 reasonings,

reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

At a COUNCIL, January 28, 1773.

Reſolved, That after Volume LXII. the *Philoſophical Tranſactions* be publiſhed twice in a year; the firſt publication to be of the months of November and December of the preceding year, and January and February of the current year, as ſoon as may be after February, under the name of the "firſt part" of the volume: and the ſecond publication to be of the remaining months unto the receſs of the Society, as ſoon as may be after the receſs, under the name of the "ſecond part" of the volume.

CON-

CONTENTS

TO

VOL. LXII.

I. *A LETTER from* James Badenach, *M. D. to* Mathew Maty, *M. D. Sec. R. S. containing a technical Description of an uncommon Bird from* Malacca. p. 1

II. *Investigation of the specific Characters which distinguish the Rabbit from the Hare: In a Letter to* Samuel Wegg, *Esq; T. and Vice-President of the R. S. from the Honourable* Daines Barrington, *V. P. R. S.* p. 4

III. *An Account of the sulphureous mineral Waters of* Castle-Loed *and* Fairburn *in the County of* Ross; *and of the salt purging Water of* Pitkeathly, *in the County of* Perth *in* Scotland: *By* Donald Monro, *M. D. Physician to the Army, and to St.* George's *Hospital, Fellow of the Royal College of Physicians, and of the Royal Society.* p. 15

IV. *Extract,*

IV. *Extract of a Letter from Mr.* George Wichell, *F. R. S. and Master of the Royal Academy at* Portsmouth, *to* Charles Morton, *M. D. Sec. R. S. inclosing some Account of a Solar Eclipse observed at* George's Island, *by Captain* Wallis; *and several Astronomical Observations made at* Portsmouth. p. 33

V. *Extract of Mr.* T. Barker's *Register at* Lyndon *in* Rutland, *in a Letter to* James West, *Esq. Pres. R. S.* p. 4?

VI. *Directions for using the common Micrometer, taken from a Paper in the late Dr.* Bradley's *Hand-Writing; communicated by the Rev.* Nevil Maskelyne, *Astronomer Royal, and F. R. S.* p. 46

VII. *A Letter from Mr.* John Reinhold Forster, *F. R. S. to* William Watson, *M. D. giving some Account of the Roots used by the* Indians, *in the Neighbourhood of* Hudson's-Bay, *to dye Porcupine Quills.* p. 54

VIII. *An Account of a Subærated Denarius of the* Plætorian *Family, adorned with an* Etruscan *Inscription on the Reverse, never before published or explained. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Custos Archivorum of the University of* Oxford, *Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany. p. 60

IX. *A Deduction of the Quantity of the Sun's Parallax from the Comparison of the several Observations of the late Transit of* Venus, *made in* Europe, *with those made in* George Island *in the* South Seas: *Communicated by Mr.* Euler, *jun. Secretary*

Secretary of the Imperial Academy of Petersburgh; *in a Letter to* Charles Morton, *M. D. Sec. R. S. & Acad. Imper.* Petroburg. *&c. Soc.* p. 69

X. *A Letter to the Rev. Mr.* Maskelyne, *Astronomer Royal, F. R. S. accompanying a new Chart of the* Red Sea, *with two Draughts of the Roads of* Mocha *and* Judda, *and several Observations made during a Voyage on that Sea, by Capt.* Charles Newland. p. 77

XI. *Remarks and Observations made on board the Ship* Kelsall, *on a Voyage to* Judda *and* Mocha, *in* 1769, *by* the same. p. 79

XII. *An easy Method to distill fresh Water from Salt Water at Sea*; *by* the same. p. 90

XIII. *Observations on the milky Appearance of some Spots of Water in the Sea*; *by* the same. p. 93

XIV. *A Letter from Mr.* Peter Dollond, *to* Nevil Maskelyne, *F. R. S. and Astronomer Royal*; *describing some Additions and Alterations made to* Hadley's *Quadrant, to render it more serviceable at Sea.* p. 95

XV. *Remarks on the* Hadley's *Quadrant, tending principally to remove the Difficulties which have hitherto attended the Use of the Back-observation, and to obviate the Errors that might arise from a Want of Parallelism in the two Surfaces of the Index-Glass. By* Nevil Maskelyne, *F. R. S. Astronomer Royal.* p. 99

* XV. *Account of the Irruption of* Solway Moss *on* December 16, 1772; *in a Letter from Mr.* John Walker, *to the Earl of* Bute, *and communicated by his Lordship to the Royal Society.* p. 123

XVI. *A Letter from* John Zephaniah Holwell, *Esq; F. R. S. to* John Campbel, *Esq; F. R. S. giving an Account of a new Species of Oak.* p. 128

XVII. *An Account of the Death of a Person destroyed by Lightning in the Chapel in* Tottenham-Court-Road *and its Effects on the Building; as observed by Mr.* William Henly, *Mr.* Edward Nairne, *and Mr.* William Jones. *The Accouut written by Mr.* Henly. p. 131

XVIII. *A Letter from* Thomas Ronayne, *Esq; to* Benjamin Franklin, *LL. D. F. R. S. inclosing an Account of some Observations on Atmospherical Electricity; in regard of Fogs, Mists, &c. with some Remarks; communicated by Mr.* William Henley. p. 137

XIX. *Observations on different Kinds of Air: By* Joseph Priestley, *LL. D. F. R. S.* p. 147

XX. *An Essay on the periodical Appearing and Disappearing of certain Birds, at different Times of the Year. In a Letter from the Honourable* Daines Barrington, *Vice-Pres. R. S. to* William Watson, *M. D. F. R. S.* p. 265

XXII. ΚΟΣΚΙΝΟΝ ΕΡΑΤΟΣΘΕΝΟΥΣ, or, the Sieve of Eratosthenes. *Being an Account of his Method of finding all the Prime Numbers; by the Rev.* Samuel Horsley, *F. R. S.* p. 327

XXIII. *A Letter from Mr.* Christopher Gullet *to* Mathew Maty, *M. D. Sec. R. S. on the Effects of Elder, in preserving Growing Plants from Insects and Flies.* p. 348

XXIV. *A*

XXIV. *A Letter from* John Call, *Esq*; *to* Nevil Maskelyne, *F. R. S. Astronomer Royal, containing a Sketch of the Signs of the Zodiac, found in a Pagoda, near Cape* Comorin *in* India. p. 353

XXV. *An Account of the Flowing of the Tides in the* South Sea, *as observed on board of His Majesty's Bark the* Endeavour, *by Lieut.* J. Cook, *Commander, in a Letter to* Nevil Maskelyne, *Astronomer Royal, and F. R. S.* p. 357

XXVI. *An Account of a new Electrometer, contrived by Mr.* William Henly, *and of several Electrical Experiments made by him, in a Letter from Dr.* Priestley, *F. R. S. to Dr.* Franklin, *F. R. S.* p. 359

XXVII. *Meteorological Observations at* Ludgvan *in* Mount's Bay, Cornwall, 1771: *By* William Borlase, *D. D. F. R. S. Communicated by Dr.* Jeremiah Milles, *Dean of* Exeter, *and F. R. S.* p. 365

XXVIII. *Account of several Quadrupeds from* Hudson's Bay, *by Mr.* John Reinhold Forster, *F. R. S.* p. 370

XXIX. *An Account of the Birds sent from* Hudson's Bay; *with Observations relative to their Natural History; and* Latin *Descriptions of some of the most uncommon. By* the Same. p. 382

XXX. *Geometrical Solutions of three celebrated Astronomical Problems, by the late Dr.* Henry Pemberton, *F. R. S. Communicated by* Matthew Raper, *Esq*; *F. R. S.* p. 434

XXXI. *On the Digestion of the Stomach after Death, by* John Hunter, *F. R. S. and Surgeon to St.* George's *Hospital.* p. 447

XXXII. *Experiments and Observations on the Waters of* Buxton *and* Matlock, *in* Derbyshire, *by* Thomas Percival, *of* Manchester, *M. D. F. R. S.* p. 455

XXXIII. *Some Account of a Body lately found in uncommon Preservation, under the Ruins of the Abbey, at St.* Edmund's-Bury, Suffolk; *with some Reflections upon the Subject: By* Charles Collignon, *M. D. F. R. S. and Professor of Anatomy at* Cambridge. p. 465

XXXIV. *A Letter from* Richard Pulteney, *M. D. F. R. S. to* William Watson, *M. D. F. R. S. concerning the medicinal Effects of a poisonous Plant exhibited instead of the Water Parsnep.* p. 469

XXXV. April 21, 1772. *Experiments on two Dipping-Needles; which Dipping-Needles were made agreeable to a Plan of the Reverend Mr.* Mitchell, *of* Thornhill *in* Yorkshire, *and executed for the Board of Longitude, by Mr.* Edward Nairne *of* Cornhill, London. p. 476

PRESENTS

PRESENTS

MADE TO THE

ROYAL SOCIETY

In the YEAR 1772;

WITH

The NAMES of the DONORS.

	Donors Names.	Presents made.
Jan. 10.	Rev. Nevil Maſkelyne, Aſt. R.	GiuſeppeToaldo, Saggio Meteorologico della vera influenza degli Aſtri. 4°
23.	Donald Monro, M. D.	Specimens of Salts, Earths, &c. from Mineral purging Waters in North Britain.
	Mr. J. Reinhold Forſter.	Voyage round the World, tranſlated from the French of Mr. de Bougainville. 4°
Feb. 6.	Mr. T. S. Kuckhan,	Specimens of Flies and other Inſects from Jamaica.
	Wm. Jones, Eſq;	Hiſtoire de Nadir Chah, traduit d'un M. S. Perſan. 4°
	M. Bouchard.	Eſſai ſur la Poëſie Rhythmique. 8°
	———	De l'Impot du vingtimee ſur les Succeſſions, chez les Romains. 8°

		Donors Names.	Presents made.
Feb.	6.	Tho. Percival, M. D.	Essays Medical and Experimental. 8°
		Rd. Watson, Th. R. Pr.	A Plan of a Course of Chemical Lectures. 8°
		Mr. Wm. Hewson.	An experimental enquiry into the properties of the Blood. 12°
		R. P. Bertier.	Principes Physiques, &c. 12°
		M. de la Lande.	Connoissance des temps pour 1772, 1773, 2 tom. 8°
	13.	Benj. Booth, Esq;	A collection of Butterflies from Fort St. George, East India.
		Richard Price, D. D.	Observations on reversionary Payments, edit. 2. 8°
		M. F. D. Michelotti.	Sperimenti Idraulici, 2 vol. 4°
	20.	Ac. El. Palatin.	Historia et Commentationes, Vol. II. 4°
		Geo. Scott, LL. D.	Camden's Britannia, translated by Edm. Gibson, Bishop of London, Edit. 1772, 2 Vol. Fol.
		Mr. J. Miller.	System of Botany, coloured Prints, N° 4. Fol.
	27.	J. C. Lettsom.	The natural history of the Tea tree. 4°
		———	The Naturalist's and Traveller's companion. 8°
Mar.	5.	Adol. Murray.	Fundamenta Testaceologiæ. 4°
		Cha. de Geer.	Memoires pour servir à l'histoire des Insectes, 2 vol. 4°
		Ac. R. Stockholm.	Their Transactions, from July 1770, to June 1771, 4 vol. 8°
	12.	Miss Blackbourne.	Specimens of Birds from N. America, 50.
		Mr. William Browne.	A dialogue on Painting, from the Italian of Lud. Dolce. 8°
		Execut. of late Mr. Hyde.	Some manuscript papers on Tides.
		John Ellis, Esq.	Two large Bezoars taken from a Miller's Horse.
	19.	Joseph Priestley, LL. D.	The History and present State of Discoveries relating to Vision, Light, and Colours. 4°
		Felice Fontana.	De i Moti dell' Iride. 4°
		Christ. Mayer.	Directio Meridiani Palatini 4°
	26.	Mr. White.	A fine Specimen of the Phœnicopterus, or Flamingo.
Apr.	2.	Prof. Titius.	Phil. Trans. for 1755, Wittemberg 1770, from the Lond. Edit. 4°
		James Ferguson.	Astronomy explained upon Sir If. Newton's principles. 8°
		———	Lectures in Experimental Philosophy. 8°

Donors

		Donors Names.	Presents made.
Apr.	2.	James Ferguson.	Tables and Tracts on Arts and Sciences. 8°
		————	An easy Introduction to Astronomy. 8°
		————	An Introduction to Electricity. 8°
	9.	Alex. Dalrymple, Esq;	A collection of Charts and Memoirs. 4°
		William Jones, Esq;	Poems from the Asiatic languages. 8°
	30.	Alex. Dalrymple, Esq;	Prints of six Charts and Maps relating to E. India.
		Coll. of Physicians.	Medical Transactions, vol. II. 8°
		J. F. Meckel.	Tractatus de Morbo hernioso congenito. 8°
		————	Nova Experimenta et Observationes de finibus Venarum ac Vasorum Lymphaticorum. 8°
		Edw. Bancroft.	An Essay on the Natural History of Guiana. 8°
		Nath. Hulme.	A Treatise on the Puerperal Fever. 8°
		————	Libellus de Scorbuto, and to prevent the Scurvy in Ships. 8°
		Don Jorge Juan.	Examen Maritimo-Theorico-Practico, 2 vol. 4°
May	7.	Mr. J. Belchier.	A half length Portrait of Mr. J. Flamsteed, Astronomer Royal.
		M. de Chamousset.	Lettres sur l'usage d'une Nouvelle découverte de Pates, &c. 8°
		Felice Fontana.	Nuove Osservazioni sopra i globetti rossi del Sangue. 8°
		Vito Caravelli.	Elementi di Mathematica, per uso della Ac. Milit. 11 vol. 8°
May	14.	Felice Fontana.	Ricerche Fisiche sopra il Veleno della Vipera. 8°
	21.	Carol. Barletti.	Nova inventa Electrica, one sheet. Fol.
		Mr. John Hunter.	The Natural History of the Human Teeth. 4°
		John Call, Esq.	Several Papers of the late Mr. Benj. Robins.
May	28.	Wm. Ger. de Brahm.	The Atlantic Pilot. 8°
		J. Reinhold Forster.	Method of assaying and classing Mineral Substances. 8°
		Dr. Andrew Duncan.	Elements of Therapeutics. 8°
		————	Observations on the Operation and Use of Mercury. 8°
June	4.	Joan. Sajnovic.	Demonstratio Idioma Ungarorum et Lapponum idem esse. Fol.
		Jean Bernoulli.	Recueil pour les Astronomes, tom. I. 8°
		————	Lettres Astronomiques. 8°

Donors

		Donors Names.	Presents made.	
June	4.	John Lawson.	Two books of Apollonius Pergæus on Tangencies.	4°
	18.	A. M. Lorgna.	De modi di migliorare l'Aria di Mantoua.	4°
		————————	Ricerche intorno alla distributione delle velocita nelle sectioni de fiumi.	
		————————	Dissertatione, &c.	
		M. de la Lande.	Memoire sur le passage de Venus, 1769.	4°
		M. Romé Delisle.	Essai de Crystallographie.	8°
July	9.	James Burrow, Esq;	An Essay upon typographical pointing, new edit.	4°

PHILOSOPHICAL TRANSACTIONS.

I. *A Letter from* James Badenach, *M. D. to* Mathew Maty, *M. D. Sec. R. S. containing a technical Description of an uncommon Bird from* Malacca.

SIR,

Read Jan. 9, 1771.

I Here present you a curious and uncommon species of bird, which I met with at Malacca in August 1770. The male, female, and two young ones, were purchased at that place from the natives, but died soon afterwards on

board of ſhip, in the paſſage from that port to China. The character and hiſtory of this bird, as they then occurred to me, and were immediately noted down in my journal, in the manner of the great Linnæus, are as follows.

T A B. I.

Mas. Magnitudo perdricis vulgaris, corpus ſupra vireſcens, ſubtus nigricans, remiges primores griſeæ, cauda brevis rotundata, apice nigro. Frons calva, criſta coccinea ex occipite orta, è 15 circiter plumulis ſeſquipollicaribus, reſpectu capitis, ſuberecta, divaricata. Roſtrum convexum breve; mandibula ſuperior nigra, ſupra inferiorem, margine rubro cerato, imbricata; nares oblongæ, orbitæ rubræ, oculi purpurei; ad baſin roſtri myſtaces aliquot albeſcentes. Femora ſeminuda, tibiæ longæ, rubræ, graciles. Pedes tetradactyli, fiſſi incarnati, ſubnodoſi, digitus poſticus reliquis craſſior, brevior, truncatuſque.

Fœmina. Mare paulo minor, criſta nulla, remiges primores & tectrices alarum ruſo ferrugineæ.

Fœmina. Mare paulo minor, criſta nulla, remiges primores & tectrices alarum ruſo ferrugineæ.

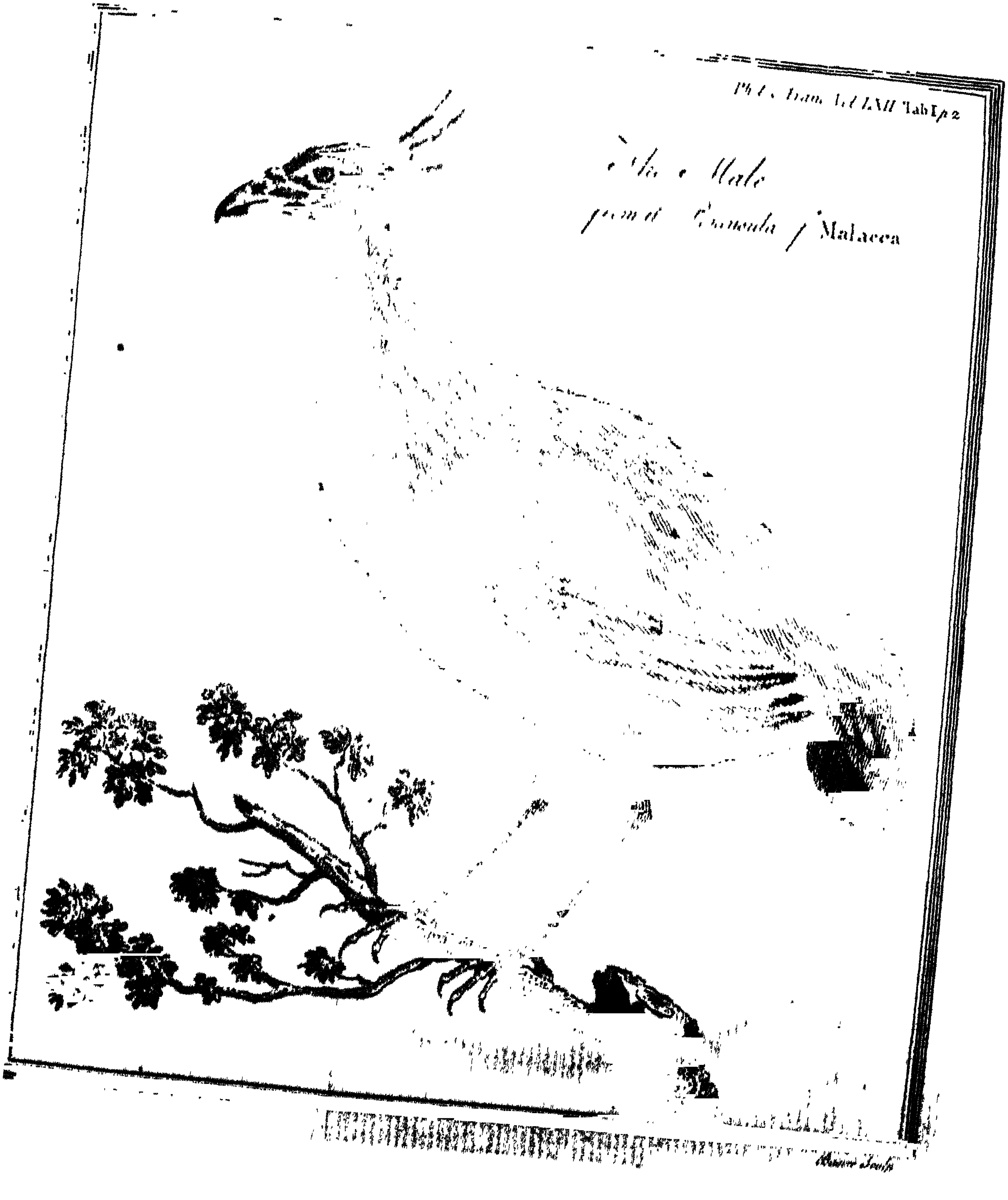
Pulli. Pulli lanuginoſi atri, aqua delectantur.

Vox. Mas & fœmina voce ſonora frequenter ſibilant.

Nidus inter gramina, arundineſque.

Victus. Oryza, pane in aqua miſſa veſcitur.

What



Phil. Trans. Vol. LXII Tab. I p. 2
The Male
from Cumala of Malacca

What genus this bird is properly to be referred to, I ſhall not pretend to determine; but if you think this, though but imperfect account, worth the communicating to your Society, you have my leave; and I am, with reſpect,

S I R,

Your moſt obedient,

and moſt humble ſervant,

London,
October 13, 1771.

James Badenach, M. D.

II. *In-*

II. *Investigation of the specific Characters which distinguish the Rabbit from the Hare: In a Letter to* Samuel Wegg, *Esq; T. and Vice-President of the R. S. from the Honourable* Daines Barrington, *V. P. R. S.*

November 21, 1771

DEAR SIR,

Read Jan. 16, 1772.

I CALLED lately at your house, to measure some parts of the quadruped which you have received by the last ship from Hudson's Bay, and am now convinced that it should be rather considered as a Hare than as a Rabbit, which latter name it hath obtained in that part of the world.

You will find indeed, from what I am going to state, that it is not very easy to settle a specific, and at the same time scientific difference, between these two animals, even when the greatest authorities in Natural History are consulted.

Ray [a] makes the distinction between the Hare and the Rabbit to consist in the smaller size of the latter, its property of burrowing, and the greater whiteness of the flesh when dressed: he chiefly relies, however, on the one being larger than the other;

[a] Syn. Quadr. Art Lepus.

as this is the moſt material circumſtance in which they are ſuppoſed by him to vary, whether exterior or interior.

Though bulk is undoubtedly a very proper circumſtance to be attended to in the deſcription of an animal; yet recourſe ſhould never be had to it in eſtabliſhing a ſpecific difference, except it is the only criterion which can be fixed upon, and the diſproportion in point of ſize is very great.

An Hare, however, does not exceed a Rabbit ſo much in bulk as a Patagonian does a Laplander, or a maſtiff does a lap-dog, which yet are not to be conſidered as differing in ſpecies.

Beſides this, age, climate, and food, as well as other circumſtances, often occaſion great diſtinction between animals of the ſame ſpecies, in point of bulk.

The Hare (for example) which is found in moſt parts of North America, is a third leſs than the European Hare, and conſequently is ſcarcely larger than our Rabbit [b].

The next criterion which Ray fixes upon to diſtinguiſh the Rabbit from the Hare, is that the latter burrows in the ground; this, however, only holds with regard to the warren Rabbit, for thoſe which are called hedge Rabbits ſeldom burrow, and many of them ſit in forms as Hares do.

[b] Monſ. de Buffon is miſinformed, when he aſſerts, on the contrary, that the American Hare is larger than that of Europe, (Hiſt. N. t. VI. p. 246) if I can depend upon the accounts I have received from thoſe who have long reſided in America, as well as ſome ſtuffed ſpecimens which I have examined. See alſo Mr. Pennant's Syn. of Quadr. p. 249.

The

The third and laſt is, that the fleſh of the Rabbit is more white when dreſſed; which diſtinction is always to be found between the European Hare and Rabbit, but it does not often happen that one can dreſs the fleſh of an animal which comes from another part of the globe; it is therefore a criterion we can ſeldom have recourſe to.

Linnæus, thus deſcribes the Rabbit in his Fauna Suecica. (Art. LEPUS).

Lepus Caniculus, cauda abbreviata, *Auriculis Nudatis*.

Lepus *cauda breviſſima, pupillis rubris.*

With regard to the firſt circumſtance of the Cauda abbreviata, he equally applies it to the Hare in his Syſtema Naturæ, publiſhed in 1766, and drops the Cauda breviſſima of the Fauna Suecica; where in propriety the Rabbit ſhould not have found a place, as it is not indigenous in Sweden, the climate being too cold for it.

Linnæus therefore could only have deſcribed from a tame Rabbit, which I ſuppoſe had balder Ears by ſome accident than common, as his next criterion is *Auriculis Nudatis*.

I have examined lately a great number of Rabbits, and do not find that their ears are balder than thoſe of a Hare: this ſecond circumſtance therefore eſtabliſhes no ſpecific difference.

From the third and laſt particular which this great Naturaliſt relies upon, I am alſo convinced that the ſpecimen before him was not only a tame Rabbit, but that its fur was either white or carroty, becauſe Rabbits of theſe colours only have red pupils [c].

[c] I have examined a great number of Rabbits thus coloured, which commonly have red pupils, though I have ſeen ſome

I find

I find accordingly, that Linnæus hath omitted the *pupillis rubris*, as applied to the Rabbit, in the twelfth edition of his Syſtema Naturæ; but adds another diſtinction, which will be found equally to fail.

He there ſays, that the ears of a Rabbit are ſhorter than the head; whereas thoſe of a Hare are longer: which is a juſt obſervation, when the warren Rabbit is examined; but the tame Rabbit (and particularly thoſe which are white or carroty) have ears that are conſiderably longer than their head.

This circumſtance, therefore, eſtabliſhes no more a ſpecific difference between the Rabbit and the Hare, than the greater length of the ears of a dog would, which in ſome varieties of that animal are known to be exceſſively long.

Monſ. de Buffon, in his deſcription of the Hare and Rabbit, agrees with Ray that there is nothing either exterior or interior which ſeems to conſtitute a ſpecific difference, though he endeavours to eſtabliſh an inconteſtable proof that they are really diſtinct.

He informs us, that he had tried to procure a breed between Rabbits and Hares, but never could ſucceed in the experiment.

This moſt ingenious and able writer does not ſtate, however, at what ages the Hares or Rabbits were thus confined, which is known to be a moſt material

with black: the grey Rabbit however never hath eyes of a red colour. When the white Rabbits are very young, their eyes are often like a ferret's; but when they are grown to their full ſize, the pupils are generally quite red.

circumſtance, by thoſe who have raiſed male Canary birds [d].

Monſ. de Buffon's expreſſion is, " J'ai fait *élever* " des haſes avec des lapins," which at firſt ſeems to imply that he had reared them from their earlieſt infancy.

Upon conſulting however the dictionary of Trevoux, the compilers inform us the word *Elever* [e] often ſignifies the feeding and keeping an animal, without reſpect to its age; and they cite its being applied to elephants in Europe, which it is believed never bred in that quarter of the globe.

But the beſt expoſitor of the ſenſe in which an author uſes a word is in other parts of the ſame work.

In the fifth Vol. of his Natural Hiſtory, p. 210. Monſ. de Buffon gives an account of his making the ſame ſort of experiment between the Wolf and a Dog, in the following words:

" J'ai fait *élever* une louve priſe dans les bois, " de deux ou trois mois."

In this paſſage, the word is applied to a wolf, of three months old, and to ſhew that Monſ. de Buffon did not think the age at which the animal is confined to be material in ſuch an experiment, he immediately afterwards ſtates, that he caught ſome

[d] Birds which differ ſpecifically ſcarcely ever breed except both are taken early from the neſt, and particulerly the hen; I have procured a breed from two robins in a cage the preſent year by attending to this circumſtance, and I believe I could equally ſucceed with almoſt any other kind of birds, as when they are thus reared, they have not the leaſt awe of man.

[e] " Elever ſignifie, *Nourrir* auſſi, ſoit plante, ſoit animal, " *& en avoir ſoin.*"

" On a de la peine à *élever des elephans* en Europe."

foxes

foxes in ſnares (which were probably therefore full grown), and kept them a conſiderable time with dogs of different ſexes.

After this, he ſays [e], it is evident from theſe experiments, that wolves, foxes, and dogs are ſpecifically different, without diſtinguiſhing between the foxes being full grown when caught, and the wolf which was only three months old.

But the deciſive argument againſt Monſ. de Buffon's experiment not being ſatisfactory, is to be found in Mr. Pennant's Synopſis of quadrupeds, p. 144: where he informs us, that a breed was actually procured between a dog and a wolf at Mr. Brooks's (animal merchant) in Holborn.

Monſ. de Buffon alſo ſuppoſes that the Rabbit is much more ſagacious than the Hare, becauſe, both having equal powers of burrowing, the one thus ſecures himſelf from moſt enemies, whilſt the other, by not taking the ſame precaution, continues liable to their attacks.

There are, however, ſeveral cauſes for the Rabbit's burrowing, and the Hare's neglecting to do ſo.

In the firſt place, the fore-legs of a Rabbit are ſhorter in proportion to its hind legs, and at the ſame time much ſtronger; the claws are alſo longer and ſharper, reſembling much thoſe of a mole.

I have before obſerved that the Rabbits, which the ſportſmen call Hedge Rabbits, ſeldom burrow; and they neglect taking this trouble, for the ſame reaſon that induces the Hare to truſt to her form, be-

[e] Hiſt. Nat. T. v. p. 213.

cause they have an opportunity of selecting a proper place for their concealment.

The ground, however, in a warren, is eaten so very bare by Rabbits, that it is impossible for them to hide themselves if they make a form in any part of it, and they therefore very judiciously choose to burrow under ground.

Another reason, perhaps, for the Rabbit's burrowing arises from the animal's being not only born, but continuing the first six weeks of its life, under ground; they therefore only practise what they have seen and learned in their earliest infancy, as birds from the same circumstance always build their nest in the same form, and with the same materials.

I therefore cannot allow entirely of the distinction arising from the superior sagacity of the Rabbit, because it burrows; and Monf. de Buffon himself informs us, that tame Rabbits turned into a warren do not burrow for many generations [*f*].

Having thus endeavoured to shew that no proper criteria have hitherto been fixed upon to distinguish the Rabbit from the Hare, I shall now venture to suggest the two following, which, I flatter myself, will be found less liable to the same exceptions.

If the hind legs of an European Hare are measured from the uppermost joint to the toe, the number of inches will turn out to be just half of the length of the back, from the rump to the mouth (the tail not being included).

The hind legs of the Rabbit being measured in the same manner, and compared with the back are not much more than one third; from which

[*f*] Hist. Nat. T. 5. p. 306.

is

it ſeems not unfair to conſider any animal of the Hare genus, (whoſe legs thus meaſured are leſs than the half of the diſtance from the rump to the mouth) as a Rabbit, and on the contrary when they are either one half, or more, as a Hare.

If the fore and hind legs of a Rabbit and Hare are alſo reſpectively compared, it will be found that the fore legs of the former are proportionally more ſhort, than thoſe of a Hare.

By both theſe criteria the quadruped from Hudſon's Bay muſt rather be conſidered as a Hare, than a Rabbit (as it is called in that part of the world), according to the admeaſurements ſubjoined, which include the reſpective proportions alſo of the Alpine Hare [*b*].

	Fore Leg. Inches.	HindLeg. Inches.	Back and Head. Inches.
Rabbit	$4\frac{1}{4}$	$6\frac{3}{4}$	$16\frac{1}{2}$
Hare	$7\frac{3}{4}$	11	22
Hudſon's Bay Quadruped	$6\frac{1}{8}$	$10\frac{5}{8}$	18
Alpine Hare	$6\frac{1}{4}$ From the uppermoſt joint to the toe.	$10\frac{5}{8}$ From the uppermoſt joint to the toe.	22

From the proportion of theſe parts, in the Hudſon's Bay quadruped, according to this table, I flatter myſelf, that it may with greater propriety be claſſed as belonging to the Hare ſpecies, than by any other marks of a ſpecific difference which have been hitherto relied upon.

[*b*] This ſpecies of Hare is found in the Highlands of Scotland, whence I received a ſpecimen, which I had the honour of preſenting to the Muſeum of the Royal Society.

I do not mean, however, to aſſert from this, that a Hare and Rabbit are certainly of a diſtinct ſpecies; as this can only be ſettled by failing to procure a mixed breed between the animals after repeated experiments, and under proper circumſtances.

I ſhall now add, that the Hudſon's Bay animal alſo approaches nearer to the Hare than the Rabbit, by the fore legs being much more ſlender in proportion to the hind legs than thoſe of a Rabbit are; and that the claws are alſo ſhorter. As the animal likewiſe happened to die on the 22d of this month, I boiled the fleſh, by your permiſſion, which was as brown as that of the European Hare; and conſequently it is to be claſſed as of that ſpecies, according to Ray's third criterion.

But the moſt curious particular in this quadruped is the white winter coat, which covered, at its death, the greateſt part of the animal.

This refutes at once the notion, that animals in the more Northern countries become white by the intenſe cold of the climate, becauſe this quadruped arrived in England about the time that the change ſhould have begun if it had continued in Hudſon's Bay.

As the animal was born, however, in a country where ſnow covers the ground during the whole winter, it is providential that the formation of its parts and juices ſhould be ſuch as ſhould periodically occaſion ſuch a change in the fur; and perhaps, it is the only quadruped which ever was brought from a climate of ſuch rigour, to a more temperate one, before the alteration of the colour in its hair took place.

By four different ſpecimens in that valuable collection of animals, which the directors of the Hudſon's Bay Company have lately procured from a country unviſited but by their own ſervants, it appears that the change begins in October (or perhaps the latter end of September) and that it is compleated in January.

We owe this knowledge of the regular gradations of colour in this animal at different intervals as the winter advances, to the very ſenſible attention in the company's ſervants, who have tied memorandums to the ſpecimen of each animal, which inform us of the day and month in which it was caught.

If the fur of your quadruped is accurately examined, it will be found to conſiſt of two diſtinct coats of hair, one of which is much more thinly ſcattered over the body, but is more than twice the length of the inmoſt covering, at the ſame time that it is vaſtly ſtronger.

This upper and thinner coat is compoſed alſo of hairs which are white from the top to the root, and form the winter ſurtout for the animal: its brown fur, therefore, never becomes white, but is concealed by the upper coat.

This additional covering ſeems to be abſolutely neceſſary for the animal's preſervation, as it is thereby enabled to endure the rigour of a Hudſon's Bay winter, whilſt at the ſame time the colour of the new fur being white, prevents its being diſtinguiſhed by its very numerous enemies [g].

If this ſurtout, however, was not to fall off during the ſummer, it would prove the deſtruction of the

animal

animal: becaufe the extraordinary heat from fuch a warm cloathing would not only become highly inconvenient, but the colour alfo (being white) would point the animal out to its purfuers; as Mr. Graham takes notice in his very accurate catalogue, that this quadruped does not burrow.

This very fenfible officer of the Hudfon's Bay Company likewife adds, that this animal continues always near the fame fpot; that its coat is brown in fummer; that they breed from 5 to 7 young ones, and fometimes twice a year: he alfo ftates, that the weight at a medium is nearly 4 lb. I am,

DEAR SIR,

Your moft faithful,

humble fervant,

Daines Barrington.

[g] It muft be admitted, however, that the white coat during the winter is not a fufficient protection to the animal againft the fagacity of the arch-enemy man.

Mr. Reinhold Forfter (who is a native of Polifh Pruffia) informs me that Hares are found in the northern parts of Europe, when the fnow is on the ground, by an exhalation of vapours from their bodies, whilft they are fitting in their form, efpecially if the fun happens to fhine.

I can very eafily conceive that fuch a vapour may be diftinguifhed, as I have frequently in a frofty morning feen the air condenfed, which hath iffued from the mouth of fo fmall a bird as a Robin, when in full fong.

III. An

III. *An Account of the sulphureous mineral Waters of* Castle-Loed *and* Fairburn, *in the County of* Ross; *and of the Salt purging Water of* Pitkeathly, *in the County of* Perth, *in* Scotland: *By* Donald Monro, *M. D. Physician to the Army, and to* St. George's *Hospital, Fellow of the Royal College of Physicians, and of the Royal Society.*

Read Jan. 23, 1772.

AS no account of these waters has hitherto been published, I thought that the following would be agreeable to this Society.

Of the sulphureous mineral water of Castle-Loed.

Having heard many gentlemen from the county of Ross speak of these mineral waters, I wrote to Sir John Gordon, of Invergordon, and begged the favour of him to ask some physical person in his neighbourhood to send me an account of them, and likewise some of the waters in bottles; and soon after he was so obliging as to send me six bottles of the Castle-Loed, and six of the Fairburn waters, sealed and corked,

corked, and along with them a letter from Dr. Alexander Mackenzy, dated August 9, 1771, containing the following account.

" The Castle-Loed is a strong sulphureous mine-
" ral water; when taken up from the spring, it is
" as pure and transparent as the clearest rock water;
" but if kept in an open vessel, or an ill-corked
" bottle, it soon becomes of a milky sort of foulness,
" and it loses its strong sulphureous smell in twenty-
" four hours.

" The bottom of the well, and of the channel
" which conveys its water from thence, is black, as
" if dyed with ink; and the leaves of the aller
" bushes that fall into the well, or into its channel,
" soon contract a blackish colour in the water; but
" when taken out, and dried in the sun or shade,
" appear covered with a whitish dust, which is un-
" doubtedly sulphur; for, by burning one or more
" on an ignited shovel, or clear live coal, they pro-
" duce a blue flame, and emit a very suffocating
" sulphureous smell.

" All that I can learn of the operation of this
" water, from some sensible people of credit and
" observation, who have drunk it this as well as for-
" mer seasons, is, that it very sensibly increases the
" urine, and sometimes remarkably opens the pores;
" but I do not find, from the report of any, that it
" purges, though drunk to the quantity of three,
" sometimes of four, English quarts in the day.
" Almost every person remarks, that it whets the
" appetite, and sits light on the stomach. I have
" been told by several, that they have had head-

"achs immediately after drinking their morning "bottle, but of no long duration, nor to any great "degree.

"It is impossible to say with certainty the num-"ber of cures these waters have made, or what par-"ticular cases have received most benefit from "using them; for every person in the county pre-"scribes the water for themselves, and runs to the "well, or sends for the water, for every complaint, "acute and chronic.

"I have indeed myself directed several people "with various complaints to drink them. Some "very foul faces have been quite cleared; and, at "this time, a gentleman's son, nine years of age, "with a herpes round the neck, which had proved "extremely obstinate to other means, has got a per-"fect cure by drinking and washing with them; "and his sister, a young lady of eighteen, who, from "an untoward recovery from the measles and small-"pox, fell into a sort of habitual erysipelas on the "face, head, breast and arms, is now using them, "and, I think, with evident advantage. Some foul "ulcers on the legs, and one with every appearance "of a carious thigh bone, have been perfectly cured. "And a servant-maid in my own family, who had "been for several years, periodically in the winter, "afflicted with severe rheumatic pains in her arms "and shoulders, received remarkable benefit from "this water, one summer; in so much, that the "winter succeeding she had little or none of her "rheumatic pains, and her appetite and digestion "were much improved."

So far Dr. Mackenzy. From others I have been informed, that this water has been uſed with ſucceſs in many of thoſe cutaneous diſorders commonly called ſcorbutic, and in curing the itch.

In order to diſcover the particular contents of this water, I began to examine the bottles, which had been ſent me, on Tueſday, the 10th of September, which was about five weeks after the water had been taken up from the well. The bottles were all well corked, and the tops of the bottles had been dipt into melted wax ſo ſoon as they had been corked. The water was as clear and limpid as the pureſt rock water. It had ſtill a very ſtrong ſulphureous ſmell and taſte; but it had no other but a ſulphureous taſte, and it made no impreſſion, on the tongue, of ſea or any other ſalt.

Some of it was poured into different glaſſes and tea-cups, and different things put into each.

Syrup of violets became ſlowly green.

A watery tincture of galls occaſioned no particular change of colour, but brought a variegated ſcum, of the colour of a pigeon's breaſt, to the ſurface.

A diluted ſpirit of vitriol mixed ſmoothly, and occaſioned no white cloud, nor more emotion or cloud than if it had been dropt into diſtilled water, only ſoon after a number of air bubbles collected at the bottom and ſides of the glaſs; and the ſame thing happened, when ſome drops of the ſtrong oil of vitriol were mixed with another parcel of the water.

Each drop of a ſolution of pure cryſtalliſed native foſſil alkali occaſioned a white cloud, and a white precipitate fell to the bottom of the glaſs; but each drop of a ſolution of ſalt of tartar cauſed a dark brown

brown-coloured cloud, with a precipitate of the ſame colour.

A ſhilling and a ſixpence, put into two different tea-cups, were preſently tarniſhed, and became of a very dark colour.

Each drop of a ſolution of ſilver in ſpirit of nitre, occaſioned a dark brown or blackiſh cloud, and fell in form of a black precipitate to the bottom of the glaſs.

Some very white ſaccharum ſaturni turned immediately black, and precipitated in form of a black powder to the bottom of the glaſs.

Four pounds ſeven ounces and ſix drachms, (or lxxi ounces vi drachms) were poured into a ſtone baſon, which was put on a ſand heat to evaporate with a ſlow fire.

As ſoon as the water became warm, it loſt its ſtrong ſulphureous ſmell, and there appeared ſome flakes of a dark brown light earth, which dropt to the bottom. After about one half was evaporated, a very thin pellicle was obſerved on the ſurface, which precipitated to the bottom, and when it was reduced to about a pint (lib. i.), it was filtrated through paper, and about 2½ gr. of a dark grey inſipid ſediment was ſeparated. This ſediment was compoſed of the dark coloured earthy flakes, which were obſerved ſo ſoon as the water had become warm, and of a ſmall quantity of a whitiſh, inſipid gritty matter, which had formed the very fine thin pellicle. Some of it being thrown into diſtilled water, and oil of vitriol dropt into it, an efferveſcence enſued, and the black earthy part diſſolved, though the reſt remained inſoluble; hence the firſt

D 2 part,

part, or black earth, ſhould ſeem to be of the abſorbent kind, the other an inſoluble earth, or a ſelenite. Whether the firſt earth was originally diſſolved in the water by means of air, or whether it was only light particles of earth, which had been blown into the well, and only ſuſpended, I ſhall not take upon me to determine; but, in looking at the water of another bottle, which was not uſed in this experiment, I obſerved, that although the water appeared quite tranſparent, yet that it contained ſome particles of light earth ſuſpended; however, theſe might have been originally diſſolved by means of air, but ſeparated afterwards.

The remaining water was put into a ſmall baſon, and ſet again on a ſand heat to evaporate; when it was reduced to about three ounces, a pretty firm pellicle appeared, and it was ſet in a cool place for twenty-four hours; at the end of which time, it was examined; and, beſides the pellicle which had formed on the ſurface, a thin white lamellated and granulated cruſt had formed, and attached itſelf to the bottom and ſides of the baſon. Theſe being all ſeparated, the whole was thrown into a filter; and when the water had paſſed, and the coffin was dry, there was found remaining gr. xi. of a very white, inſipid gritty ſediment. This ſediment in the mouth feels gritty, and has no taſte, being quite inſipid; when ſome of it was put into a glaſs with diſtilled water, and a few drops of oil of vitriol mixed with it, a very ſlight efferveſcence enſued; but almoſt the whole remained undiſſolved, and appeared to be ſelenite.

The

The water, being again ſet to evaporate, was reduced to leſs than an ounce, when it was again filtred, and gr. ii ſs. of a reſiduum ſeparated. This ſediment appeared to be nearly of the ſame nature as the former; but, on putting it into the mouth, I thought I could perceive ſomewhat, though very little, of a ſaltiſh taſte, and when thrown into water, it remained all undiſſolved.

The ſmall quantity of water which was left, was next evaporated to dryneſs, and there remained in the tea-cup gr. xvii. of a yellowiſh matter, compoſed of the thin lamellæ of ſome ſalt, and a yellow unctuous or oily ſubſtance. It had roſe into bliſters, and emitted a very ſtrong ſmell of ſulphur, while it yet remained hot.

Some of the ſaline matter, being put into a ſolution of the cauſtic alkali in diſtilled water, occaſioned a white cloud; and the ſame thing happened when ſome of it was mixed with ſolutions of ſilver in the nitrous acid, and of corroſive ſublimate in diſtilled water.

In order to ſeparate the ſalt from the yellow oily matter, the ſediment was thrown into and diſſolved in an ounce of diſtilled water, and then paſſed through filtring paper, and evaporated to a pellicle; after which, it was ſet in a cool place for forty-eight hours, in which time a cryſtalliſation took place, and I ſeparated very near gr. xii. of a ſalt ſimilar, in every reſpect, to that of Glauber; but it had ſtill a little of the yellow oily matter adhering to it. This ſalt had a cool bitter taſte; it diſſolved eaſily in diſtilled water, and when ſome of the native foſſil alkali was added to this ſolution, it remained clear;

though when a ſolution of the cauſtic vegetable alkali was added, each drop occaſioned a white cloud. Some drops of oil of vitriol, let fall on ſome of this ſalt, occaſioned no efferveſcence, nor raiſed any fumes; and when ſome of it was put on a red hot poker, it roſe into bliſters, and did not crackle. From all which I conclude, that this was a true Glauber ſalt.

After this ſalt was ſeparated, the remaining liquor was left in the tea-cup, and, at the end of four days, it concreted into a yellow cake, which ſtill contained a good deal of a ſalt; it weighed gr. ix. ſo that there had been a great increaſe of weight, from the water the ſalts had taken up in their cryſtalliſation. This matter was extremely bitter, and had likewiſe a cooling ſaline taſte. Some of it, put on a red-hot poker, melted, and roſe into bliſters; it emitted a little ſmoak, but did not flame; and it had ſuch a very ſlight ſulphureous ſmell, that it was doubtful whether it proceeded from the yellow matter or the ignited iron; it burnt to a black coal, which taſted ſomewhat ſaltiſh. Some oil of vitriol dropt on ſome of this matter occaſioned very little efferveſcence, but raiſed a ſtrong pungent acid ſmell, which I at firſt imagined muſt be that of ſea ſalt; but, from the few marks I have obſerved of the exiſtence of this ſalt in a perfect ſtate in theſe waters, I have ſince thought, that it might be that of a volatile vitriolic acid, which had been formed by the union of ſome of the acid of the Glauber ſalt, with a ſulphureous or oily matter, and diſlodged by the addition of a fixed acid of the ſame kind, though perhaps there may be a pittance of a calcularcous marine ſalt in

in the yellow ley mixed with the Glauber ſalt. Having accidentally added a ſolution of cauſtic alkali to this mixture of the yellow matter, and vitriolic acid, it emitted immediately a ſtrong ſmell of hepar ſulphuris. This yellow matter I take to be compoſed of a Glauber ſalt, and a yellow oily matter common to almoſt every water, though perhaps in larger proportion in this than in many others; but whether it contains a pittance of ſea ſalt, I think is doubtful; and what makes me ſtill doubt the more, is my having ſince evaporated 44 ounces of the water, from which I obtained gr. x. of reſiduum, when the water was at laſt evaporated to dryneſs, which I threw likewiſe into diſtilled water, and the moſt of it paſſed the filtre, and concreted into pure cryſtals, reſembling thoſe of Glauber ſalt, leaving but a very ſmall pittance of a yellow oily matter behind. However, it will be neceſſary to have a pretty large quantity of this laſt reſiduum, to determine with preciſion the nature of every ingredient in its compoſition, and the exact proportion of each.

Having tried as many experiments as I could, with the ſmall quantity of this yellow matter I had, I laſt of all examined the coffin through which the ſalts and it had paſſed. After it was dry, I found that it had increaſed gr. ii. in weight, and that it was covered in the inſide, with a yellowiſh powder. When part of the paper, with this powder, was lighted with a candle, and the flame extinguiſhed, it ſmelt ſtrong of ſulphur; and, on rubbing a ſhilling with another bit of the paper, it immediately tinged it yellow, as pure ſulphur would have done. From whence, I think, we have reaſon to conclude, that this powder

con-

contains more or leſs of a true genuine ſulphur, or, at leaſt, of a ſulphureous matter.

From what has been ſaid, it appears, that this is one of the ſtrongeſt ſulphureous waters hitherto found in Great Britain, though I make no doubt but that there are many ſuch which have not hitherto been examined: That, in its natural ſtate, it is highly impregnated with a volatile ſulphureous vapour, which evaporates ſoon when expoſed to the open air, and flies off immediately when expoſed to heat; and that the water then loſes its ſtrong ſulphureous ſmell and taſte, though we have the ſtrongeſt reaſon to ſuſpect that it ſtill contains a ſulphureous matter diſſolved in it, by ſome means hitherto unknown to us; for it neither contains an alkaline ſalt nor quicklime, the two only ſubſtances we hitherto know to be capable of diſſolving ſulphur, and keeping it ſuſpended in water: That it lets drop to the bottom of the well, and of its channels, a fine powder of ſulphur, which adheres to the leaves and branches of trees found there.

As this water contains but very little purging ſalt, and does not operate by ſtool, ſea water, or ſome purging ſalt, may be added to the firſt glaſſes drank in a morning, when purging is required. Equal parts of the Caſtle-Loed and ſea water mixed together, make a water in moſt reſpects ſimilar to the Harrowgate; and probably will be found to anſwer in moſt caſes where the Harrowgate has been found uſeful; and it may often be uſed with more advantage than the purging ſulphureous waters, as they ſometimes purge people of weak conſtitutions too freely, and weaken them too much.

Of

3

Of the ſulphureous Mineral Water of Fairburn.

Dr. Mackenzy in his letter mentioned no more of this than that he believed it to be a weaker water, of the ſame nature as the Caſtle-Loed.

I ſubjected it to the ſame tryals, as it: on opening the bottles, it emitted a ſtrong ſulphureous ſmell, tinged ſilver, and produced nearly the ſame appearances as the Caſtle-Loed when mixed with the ſame ſubſtances, only it remained clear when a ſolution of the true foſſil alkali was mixed with it; the cauſtic vegetable alkali occaſioned a very ſmall light, darkiſh cloud, and precipitated but a very ſmall quantity of a very light ſediment, owing, as appeared afterwards, to this water containing an abſorbent or calcarious earth, which probably was ſuſpended by air, and but very little if any ſelenite.

I evaporated lib. viii. drachm. 1. ſcrup. 1. (or 128 ounces, four ſcruples) of this water with a ſlow fire. When it was evaporated to one half, it was filtrated through paper; which operation was repeated again, when reduced to three ounces; after which the remainder was evaporated to dryneſs, and the ſolid matter left, thrown into diſtilled water, filtrated again through paper, and evaporated to a pellicle, and ſet in a cool place for the ſalts to cryſtalliſe.

By theſe operations, I obtained near gr. ii. of a dark coloured light earth, which efferveſced with acids, and diſſolved; gr. xv. of a white calcarious earth, which efferveſced with and diſſolved in the vitriolic acid;—and gr. xxiv. of Glauber ſalts mixed with a

 yellow

yellow oily matter; but I got no ſelenites, nor any matter which coloured ſilver, or I had any reaſon to ſuſpect to be ſulphur.

Some of the ſalt was diſſolved in diſtilled water, and different liquors were dropt into different parcels of it.

Syrup of violets became immediately of a green colour.

Each drop of a ſolution of ſilver in the nitrous acid, occaſioned a bluiſh white cloud, which fell to the bottom.

The ſolution of the foſſil alkali mixed clear, as did a ſolution of ſalt of tartar, but each drop of a ſolution of common cauſtic alkali gave a white cloud; ſome oil of vitriol dropt on a little of this ſalt effervеſced, and emitted acid fumes, while it yet was mixed with a good deal of the yellow oily matter; but after the ſalt was diſſolved in diſtilled water, and again cryſtalliſed, and freed of moſt of this yellow matter, no fumes were to be obſerved; and the acid ſmell was extremely faint when ſtrong ſpirit or oil of vitriol was dropt upon it.

This, though it does not appear to be ſuch a ſtrong ſulphureous water as the Caſtle-Loed, yet it may have its uſes, and be ſerviceable to thoſe who have not an opportunity of uſing the other; and it may perhaps be uſeful in ſome caſes, where the other may not agree.

Of the Salt Purging Water of Pitkeathly, in the County of Perth.

There are but few ſalt purging waters, which have hitherto been diſcovered in Scotland; the Pitkeathly,

Pitkeathly, ſituated about ſix miles from the town of Perth, is the one in moſt eſteem, and the moſt frequented.

As no particular treatiſe has been publiſhed on theſe waters, and I wiſhed to know their particular nature and contents, I wrote to his Grace the Duke of Athol, whoſe ſeat of Dunkeld is within 14 or 15 miles of the wells, and begged the favour of him, to aſk ſome of the phyſical people in the neighbourhood to examine theſe waters, and to ſend me an account of them; and in conſequence thereof, his Grace was ſo obliging as to ſend me a letter from Dr. Wood, of Perth, giving the following deſcription of them; and afterwards ſix bottles of the water, which arrived in ſpring 1771.

"The ſpring riſes in a very low marſhy ground, "undiſtinguiſhable from any other; but, by the taſte "of its water, it is generally believed to contain "no mineral principle, but a ſmall proportion of "marine ſalts. It acquires ſomewhat of a putrid "taſte by keeping, but retains its purging quality; "and it keeps much better in open, than in corked "bottles.

"It purges gently, and without griping. An "adult perſon drinks commonly a bottle and a half, "or two bottles, of a morning.

"In ſcrophulous and ſcorbutic habits, it is cer"tainly a moſt uſeful water.

"A new ſpring has been lately diſcovered about "two or three hundred yards from the old one, "but its waters ſeem to be much of the ſame "ſtrength and quality as the former."

 Since

Since receiving the above account, I wrote to Dr. Wood, and begged to know of him what proportion of fea falts thefe waters contained, and whether they had any mixture of a bittern in their compofition; and I had the following anfwer, dated Oct. 17, 1770.

"Since I received your letter, I evaporated a "Scotch pint (Lib. iv.) of thefe waters in a white "ftone bafon, and I obtained two drachms of a "falt, which always run *per deliquium*, and would "not cryftallife. I fhall try it again in the fum-"mer, as at this feafon the air, being much charged "with watery particles, may have prevented the "cryftallifation. By dropping a folution of potafh "into three Scotch pints (lib. xii.) of the waters, "I got eighty five grains of a very fine magnefia."

The fix bottles of this water which were fent to me, arriving at a time when I was much engaged, they remained for feveral months in the hamper in which they were originally packed; and I did not try any experiment with the water till the 2d of October laft. It was then clear and tranfparent as the pureft rock water, only it feemed to have fome few particles of light earth fwimming through it. It had then, a fetid fulphureous fmell, refembling fomewhat that of a foul gun or of rotten eggs, and it tinged filver in the fame way as the fulphureous waters beforementioned; and it had a fulphureous and flight faltifh tafte. This fetid fulphureous fmell, tafte, and property of tinging filver, which this as well as moft other falt waters acquire by keeping, I fufpect to be owing to a fermentation taking place in the water, and flightly uniting fome of the fine oily

matter.

matter with ſome of the acid of the ſalts which theſe waters contain, and thus forming a ſulphureous vapour which is volatile while they remain ſlightly united, but which by a more intimate union would form a real fixed ſulphur. From Dr Wood's account of this water, it is evident that this fetid vapour, or at leaſt the principles which form it, are volatile; for, he ſays, the water keeps much better in open than in corked bottles.

Each drop of a ſolution of the foſſil as well as of the vegetable alkali occaſioned a thick white cloud, that fell to the bottom of the glaſs. And each drop of a ſolution of ſilver in the nitrous acid gave a milky cloud. Syrup of violets became green, and an infuſion of galls occaſioned no particular change of colour.

A hundred and two ounces, three drachms and a ſcruple (or lib. vi. unc. vi, dr. 3. ſcrup. 1.) were put into a large ſtone baſon, and ſet on a ſand heat to evaporate with a ſlow fire.

As ſoon as the water was warm, it let drop a light dark coloured earth, which gathered in ſmall heaps at the bottom of the baſon; and during this time, the water threw up ſome air bubbles to its ſurface; when it was evaporated to about a pint (lib. i.) it was taken off the fire, and filtrated through paper: the coffin through which it paſſed, after being dryed, was found to have acquired 21 grains of additional weight; though I could not collect more than 3 grs of a ſtone grey coloured earth, which proved to be of the abſorbent or calcarious kind, for it efferveſced with and diſſolved in the vitriolic acid; the remaining additional weight of the coffin, I believe, depended on ſome of

 the

the salts of the water being taken up by the spungy filtrating paper.

After this, the water was again set on the sand heat and evaporated till a pellicle appeared on the surface; and during the evaporation it threw up a great number of air bubbles: after this, it was set in a cool place for three days, at the end of which time there appeared a quantity of thin lamellæ, mixed with a small granulated salt, covered with a light coloured yellowish liquor; these I separated, and threw the liquor into filtrating paper; and by these operations I got 53½ grains of a Salt which tasted sharp and salt, besides what had been taken up by the coffin, which had increased gr. 9. in weight more than I had got of salt. This salt being put in a tea cup appeared next day white, and had contracted a little moisture, but did not run *per deliquium*.

The remaining water which was now a yellowish ley, was again evaporated to a pellicle, and I separated a quantity of white salt in lamellæ, which remained moist, till it was set in a tea cup on the sand heat and evaporated to dryness, when it weighed one drachm and 14 grains; this salt attracted more moisture than the former, and seemed at first as if it would run soon *per deliquium*; but the next day it remained in the same state.

As I imagined that both this and the salt before separated was mostly sea salt mixed with a bittern and oily matter, which prevented the crystallisation; I dissolved the whole of both in distilled water, and evaporated with a very slow fire till a crystallisation began to appear, and then set it in a cool place, and got some large perfect crystals of sea salt, and by repeating

ing this ſeveral times, I obtained a full drachm of perfect cryſtals, which diminiſhed in their ſize as the proceſs advanced; and afterwards a ſcruple more of thin lamellæ, which on examining with a magnifying glaſs appeared to be made up of ſmall ſquare cryſtals; there remained a ſmall quantity of a ſalt ley, which probably would have yielded a few more ſuch lamellæ.

The liquor which remained after the two firſt parcels of ſalt were ſeparated, was next evaporated; but no pellicle appearing, the operation was continued till it was quite dry, when it formed one tranſparent yellow or amber-coloured ſalt cake, which weighed one drachm and 34 grains. This ſalt on being put into a tea cup, preſently began to run *per deliquium*, and diſſolved entirely by ſtanding in a cupboard which was in a room where there was a fire; but the fire having been let out in the evening, and the night proving cold, I found next morning that a cryſtalliſation had taken place, for there was a cryſtalliſed cake at the bottom of the cup, which was covered with an amber-coloured ley; it at firſt ſeemed to be all one piece, with a number of ſmall points ſtanding up on its ſurfaces; but on reclining the tea cup to a ſide, it then appeared to be made entirely up of a number of oblong cryſtals about the length of a barley-corn, but not ſo thick, and that the points beforementioned were the ends of theſe cryſtals. Not having time to examine them particularly in the morning, and to know their exact figure and number of ſides, I ſet them by, till I ſhould come home again about one o'clock; but the day proving warm, they were moſtly diſſolved before that time.

Oil of vitriol, dropt into a tea cup in which there was ſome of this ley, immediately occaſioned a white

firm

firm coagulum like chalk, which was insoluble in water, and, when well washed and freed of its acid, felt gritty and was quite insipid in the mouth; this is certainly a selenites formed by the earth of this ley and the vitriolic acid.

From this account of the Pitkeathly waters, it appears that lib. vi. unc. vi. dr. 3. scr. 1. besides a few grains of an absorbent or calcarious earth, contain three drachms, 41 ½ grains (besides what was lost in filtrating and other operations) of a saline matter, of which near two thirds were sea salt, the rest a bittern or salt with an earthy basis, which concreted by the force of fire into a yellow saline mass, that runs soon *per deliquium*, and crystallises though with difficulty.

The small quantity I had of residuum prevented my being able to determine with precision, the exact proportion of sea salt and of this bittern; neither was I, for the same reason, able to determine whether this bittern or ley was all made up of a calcarious marine, with an oily matter common to all waters, or whether it contained likewise a sal catharticum amarum with a vitriolic acid.

From the acid of vitriol forming an insoluble selenites with the earthy basis of this bittern, it is evident, that at least all the earthy basis is not a magnesia, such as makes the basis of the sal catharticum amarum of the shops, or what goes by the name of Epsom salts, otherwise it would have formed a salt easily soluble in water.

IV. *Extract*

IV. *Extract of a Letter from Mr.* George Witchell, *F. R. S. and Master of the Royal Academy at* Portsmouth, *to* Charles Morton, *M. D. Sec. R. S. inclosing some Account of a Solar Eclipse observed at* George's Island, *by Captain* Wallis; *and several Astronomical Observations made at* Portsmouth.

To Cha. Morton, Sec. R. S. &c.

SIR,

Read Feb. 13, 1771.

I Beg the favour of you to lay before the Royal Society, an extract of a letter to me from Captain Wallis, containing an observation of a solar eclipse, which happened during his stay at George's Island, from which I have calculated the difference of meridians between that place and Greenwich.

To this I have subjoined some astronomical observations, which have been made here by Mr. Bradley and myself, both before and since the building of the Observatory belonging to the Academy; which, if they should prove acceptable to the Royal Society, will induce us to take every opportunity of

continuing to tranſmit them, as we ſhall always eſteem it a peculiar happineſs to be able to contribute any thing, that may be deemed worthy of their notice.

I am, SIR,

Your moſt obedient,

humble ſervant,

Royal Academy, Portſmouth,
Auguſt 9, 1771.

G. Witchell.

Extract of a Letter from Captain Wallis, June 20th, 1771.

——— "Saturday, July 25th, 1767, being at
" anchor in his Majeſty's ſhip Dolphin in harbour,
" went on ſhore on a low point of land, not above
" four feet higher than the ſea, and obſerved an
" eclipſe of the Sun, as below. Latitude, by the
" mean of many obſervations, 17° 30′ South, lon-
" gitude, by various obſervations of the diſtance of
" the Sun from the Moon, between 149° 30′ and
" 149° 50′ Weſt from London.

" By

	h	′	″		h	′	″
" By the master's watch at	7	6	0	A. M. the ☉'s altitude was 8° 43 on the quadrant, without any correction. Hence the app. time	7	5	20.
" By my watch at . .	7	3	20				
" By the master's watch at	8	13	0	A. M. the ☉'s altitude was 22° 52′ on the quadrant, without any correction. Hence the app. time	8	12	12
" By my watch at . .	8	10	12				

	h	″	″			h	′	′
" The eclipse began, by the master's watch, at .	6	52	30	A. M.	Apparent time,	6	51	50
" By my watch, at	6	49	50					
" The end of the eclipse, by the master's watch, at	8	1	48	A. M.	. . . . .	8	1	0
" By my watch, at	7	59	0					
					" Duration	1	9	10

" We were not certain of the instant of the beginning of the eclipse, from a little negligence; but " very certain of the end."

Remark.

As the Sun's altitudes are given, without any correction, I suppose they were taken by bringing down the image of the Sun, till it appeared bisected by the visible horizon: I have therefore recomputed the time, by allowing for the dip and refraction, which, together, amount to 8′. This correction makes the apparent time of the beginning $6^h\ 51'\ 12''$, and the end $8^h\ 0'\ 37''$; hence the duration of the eclipse was $1^h\ 9'\ 25''$; but, by a careful computation from Mayer's new Tables, the duration should have been $1^h\ 13'\ 20''\frac{1}{2}$, which is almost about 4′ longer than the observation affords; but, as it is remarked that the beginning was not exactly taken, and the Moon entering very obliquely on the Sun, the defect in 4′ would be but little. It seems most reasonable to attribute the whole of the error to the beginning of the eclipse. I have therefore deduced the longitude from the end, and

 make

make it to be $9^h\ 55'\ 55''$ West from Greenwich, or $148°\ 58'\frac{3}{4}$, which is $41'\frac{1}{4}$ less than the mean result of the lunar observations, which, considering all circumstances, is not, in my opinion, a very great difference for the first observations that were ever made upon this island.

Astronomical Observations made at the Royal Academy, Portsmouth.

1769. May 9th, at $8^h\ 13'\ 9''$, apparent time, Mr. Bradley observed the immersion of ζ ♊orum by the Moon, uncertain to a few minutes, on account of the strong twilight. The emersion was not taken.

The transit of Venus, and solar eclipse, next morning, were both observed here; but, having then no better instrument for determining the going of the clock, than an indifferent Hadley's sextant, I do not think the observations worthy of being laid before the Society; and, for the same reason, omit the observations of the comet.

1770. April 7th, at $11^h\ 23'\ 33''$, apparent time, by Mr. Bradley's observation, the Moon occulted ι ♌is. My time was within $2''$ or $3''$ the same; but we did not observe the emersion. This occultation was observed both at Greenwich, by Mr. Maskelyne, and at Oxford, by Professor Hornsby; by comparing which, it appears that this place is West of Greenwich $4'\ 24''\frac{1}{2}$ of time, and that Oxford is West of Greenwich $4'\ 58''\frac{1}{2}$.

1770. April 28, at $9^h\ 48'\ 13''$, apparent time, Mr. Bradley and I, both at the ſame inſtant, obſerved the immerſion of ζ ♉ [1] by the Moon. The emerſion was not taken. By comparing this with Mr. Maſkelyne's obſervation, our longitude comes out $4'\ 23''\frac{1}{2}$ Weſt from Greenwich.

1770. July 21ſt is marked for an eclipſe of Juptiter's fourth ſatellite in the Nautical Almanac; but the Connoiſſance des Temps notes it as a conjunction only, and remarks, that the ſatellite would raſe the ſhadow, without diſappearing; which we found to be true, for we both ſaw the ſatellite the whole time which is marked for its duration, though, at the middle, it appeared extremely faint.

These obſervations were made before our Obſervatory was finiſhed; but that being completed in the month of September, and furniſhed with an excellent (though ſmall) mural quadrant and tranſit inſtrument, both made by that eminent artiſt Mr. John Bird, we began to obſerve meridian tranſits, from which I ſhall ſelect thoſe that were made for determining the ſolſtices, and the oppoſitions of the three ſuperiour planets, which I ſhall tranſcribe, juſt as they were taken, excepting only making the neceſſary allowance for the error of the line of collimation.

Observations for determining the Solstices.

			App. zen. dist. of the ☉'s U.L.			App. zen. dist. of the ☉'s L.L.			Barometer.	Thermometer.	Hence the app. zen. dist. of the Sun's center.		
			°	'	"	°	'	"			°	'	"
1770.	Dec.	9.	73	21	40.3	73	54	5.1	29.76	37	73	37	52.7
		12.	73	36	41.8	74	9	25.0	29.78	46	73	53	3.4
		14.	73	44	22.0	74	16	58.2	29.87	44+	74	0	40.1
		21.	73	56	41.0	74	29	13.1	29.37	43+	74	12	57.0
		22.	73	56	20.0	74	29	8.1	29.94	38½	74	12	44.0
		29.	73	42	27.2	74	15	0.4	29.62	48½	73	58	43.8
1771.	Jan.	3.	73	18	18.0	73	51	3.6	29.23	46	73	34	40.8
1771.	June	18.	27	5	45.4	27	37	42.2	29.82	64+	27	21	43.8
		19.	27	4	52.7	27	36	32.1	30.03	66½	27	20	42.4
		20	27	3	52.0	27	35	44.4	30.11	71+	27	19	48.2
		21.	27	3	33.6	27	35	26.0	30.12	70	27	19	29.8
		22.	27	3	51.0	27	35	29.4	30.11	67½	27	19	40.2
		24.	27	5	17.1	27	37	3.5	30.06	67	27	21	10.3
		25.	27	6	43.2	27	38	32.0	30.04	72½	27	22	37.6

° ' "

By comparing these observations together, I make the true zenith distance of the Sun's center, at the Winter Solstice, to be 74 16 13.4

And at the Summer Solstice 27 19 51.6

Therefore, the distance of the tropics 46 56 21.8

Half 23 28 10.9

By Mr. Mayer's Tables, the decrement of the obliquity, in three months, is 0.1

Hence the mean obliquity, December 21, 1770 . . 23 28 11.0

And June 21, 1771 . . . . 23 28 10.8

Therefore, the latitude of the Observatory, by these observations, is . . . . 50 48 2.4 North.

The above observations were chiefly taken by Mr. Bradley. Those which follow are what I took about the time of the late oppositions of the superior planets, in which, as well as the preceding observations, the apparent zenith distances are those which were taken by the 96th arc, on which we chiefly depend, though the difference between the two arcs seldom arises to more than three or four seconds.

For

For the Opposition of the ☉ and ♂.

Days.	Time per clock. h ′ ″	Meridian Transits.	App. zen. dist. ° ′ ″	Barom.	Therm.
1770. Dec. 10.	5 18 58½	β Leporis passed the middle wire.			
	20 51—	δ Orionis.			
	36 49—	Mars	24 41 6.0	29.87	28¾
	43 19	α Orionis.			
13.	5 12 24—	β Tauri.			
	31 35	Mars	24 38 11.4	29.76	42½
	43 21+	α Orionis.			
	6 1 37	η Geminorum.			
16.	5 26 22½	Mars	24 36 55.5	29.80	47½
	43 21	α Orionis.			
17.	5 12 24½	β Tauri.			
	24 40	Mars	24 36 15.6	29.91	43
	43 21	α Orionis.			
20.	5 4 7—	Rigel.			
	19 40:	Mars	24 36 25.6	29.16	43
	37 29+	α Orionis.			

For the Opposition of the ☉ and ♄.

Days.	Time per clock. h ′ ″	Meridian Transits.	App. zen. dist. ° ′ ″	Barom.	Therm.
1771. Febr. 1.	9 1 51+	Saturn passed the middle wire.	32 48 20.8	29.89	42
	16 26—	α Hydræ.			
2.	9 1 32	Saturn	32 46 53.4	30.07	42+
	16 26½	α Hydræ.			
7.	8 32 58+	ε Leonis.			
	59 57½	Saturn	32 39 27.6	30.04	28
	9 29 4+	α Leonis.			
10.	8 59 1½	Saturn	32 35 9.5	29.72	18¾
	9 16 32	α Hydræ.			
	29 5+	α Leonis.			

For

For the Opposition of ☉ and ♃.

Days.	Time per clock. h ′ ″	Meridian Transits.	App. zen. dist. ° ′ ″	Barom.	Therm.
1771. July 12.	16 15 41	Antares passed the middle wire.			
	18 0 21½	μ Sagittarii.			
	9 44—	η Serpentis.			
	14 7½	λ Sagittarii.			
	19 39 8½	Jupiter	72 40 43.8	29.87	57½
13.	7 28 52½	☉'s preceding limb.			
	31 8+	☉'s following limb.			
	19 38 34	Jupiter	72 41 52.5	30.18	57+
14.	7 32 53	☉'s preceding limb.			
	35 10+	☉'s following limb.			
	19 37 59	Jupiter	72 43 22	30.26	61
15.	7 36 55	☉'s preceding limb.			
	39 12	☉'s following limb.			
	16 15 35+	Antares.			
	19 37 23	Jupiter	72 44 57.1	30.12	59
17.	7 44 56—	☉'s preceding limb.			
	47 12+	☉'s following limb.			
	16 15 31	Antares.			
	19 36 14½	Jupiter	72 47 50.7	29.93	65
23.	8 8 46	☉'s preceding limb.			
	11 0½	☉'s following limb.			
	16 15 10½	Antares.			
	19 32 48	Jupiter	72 55 47.3	30.21	60½

An Occultation of λ Libræ by the Moon.

Day.	App. time. h ′ ″	
1771. April 2, at	13 7 9½	λ Libræ emerged from the Moon's limb. Mr. Bradley makes the time 3″ later. The immersion was not taken.

The

The Moon's Paſſage over the meridian, near the ☍.

Day.	Time per clock. h ′ ″	Meridian Tranſits.	App. zen. diſt. of the ☽. ° ′ ″	Barom.	Therm.
71. April 28.	2 19 54	Sun's preceding limb paſſed the ſecond wire		29·74	44
	24 4	Sun's following limb paſſed the fourth wire			
	14 17 23·4	Moon's preceding limb paſſed the middle wire	Upper limb 66 13 13·0		
	19 40·4	Moon's following limb	Lower limb 66 45 48·1		

It being very clear this evening, when the Moon paſſed the meridian, we were in hopes of getting a good obſervation of the lunar eclipſe, which happened ſoon after; but the air did not long continue in that ſtate, but became ſo hazy, that we could not get any certain obſervations.

V. *Extract of Mr.* T. Barker's *Meteorological Register at* Lyndon *in* Rutland, *in a Letter to* James West, *Esq; Pres. R. S.*

SIR,

Read Feb. 13, 1772.

I Have, according to your desire, sent you, on the other side, the quantity of rain which fell last year, and have added an abstract of my observations of the barometer and thermometer, and a general account of the weather here. And, with all proper regard,

I am, SIR,

Your humble servant,

Lyndon,
Jan. 18, 1772.

T. Barker.

Barometer.

1771.		Barometer.			Thermometer. Within doors.			Thermometer. Abroad.			Rain.
		[illegible]	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
Jan.	Morn.	[illegible]	[illegible]	[illegible]	[illegible]	29	[illegible]	[illegible]	16	27	1.415
	Aftern.				[illegible]	30	30	50½	23	31	
Feb.	Morn.	[illegible]	[illegible]	29.58	[illegible]	27	38	13	1	29	[illegible]
	Aftern.				[illegible]	30	39	[illegible]	[illegible]	39	
Mar.	Morn.	[illegible]	[illegible]8.97	29.44	46	33	30	[illegible]	18	31	[illegible]909
	Aftern.				47	34	40	[illegible]	32	39	
April	Morn.	29.[illegible]8	29.[illegible]9	29.6[illegible]	50	37	43	[illegible]	25	34	0.97[illegible]
	Aftern.				51	37½	45	63	33	46	
May	Morn.	[illegible]	29.04	[illegible]49	62	45	54½	67	41	50½	0.658
	Aftern.				64	46	56	76	52	62	
June	Morn.	[illegible]	29.10	29.65	62	52	57	59	46	[illegible]	1.583
	Aftern.				64	[illegible]	59	72	50	61½	
July	Morn.	[illegible]	29.11	29.61	63	56	61	62	51	55½	1.013
	Aftern.				70½	57	63	77	57	67½	
Aug.	Morn.	29.75	29.07	29.48	61	56	59½	62½	46½	54	2.131
	Aftern.				70½	57	61½	70½	56	65½	
Sept.	Morn.	30.00	29.28	29.62	66	53½	57	68	39	49½	1.155
	Aftern.				64	54	58	66	53	56	
Oct.	Morn.	30.[illegible]6	28.53	29.44	58	47	51	57	33	43	4.072
	Aftern.				59	48	52	62	44	52½	
Nov.	Morn.	30.2[illegible]	29.03	29.72	52	40	15	54½	26	38	0.792
	Aftern.				53½	41	46	56	31½	44½	
Dec.	Morn.	29.93	28.34	29.22	50	40	44	49½	25½	38½	1.927
	Aftern.				49½	40	44	50	35½	44	
											17.58[illegible]

That very wet ſeaſon, the laſt quarter of 1770, ended about Chriſtmas, and except three or four warm days, with ſome thunder, the beginning of January 1771, it was froſty above another quarter of a year. No froſt, indeed, continued ſteady much above a fortnight together, but they were remarkably ſharp, (particularly February 12, the Thermometer abroad was lower than I have ſeen it in above twenty

years.)

years.) The intervals between the frosts were short, and often frosty mornings, and a settled frost as late as the end of March; so that there were but few mornings, till April 20, but were more or less frosty. The effects were, that garden-things, turnips, &c. were very much destroyed; bays, arbutus, myrtles, fig-trees, and other tender things, were killed down to the ground, and even most of the common furz; and there were scarce any signs of spring to that time, and the winter corn was very thin.

Then the weather grew milder, and in May warm, and there came a pleasant, but cool and dry, summer, and often windy; so that the grass was short, and the crops of hay small: but whether it was from the ground having been so soaked in winter, the coolness of the summer, or two fine rains in the middle of June and August, the ground was never so much burnt as it sometimes is. Every thing was, and continued, very backward; the hottest part of this summer was the middle of July; after which, though there were several fine showers, the ground continued to burn till toward the middle of August, when some rains made the grass to grow again; yet it began rather to burn again, in some places, in September. The harvest was very late this year, especially the wheat, which both eared and ripened after the barley, and most of it was reaped in September. Both hay and harvest were well got in, and the crops were well eared, but much of the wheat and rye continued very thin; which was too much to be feared, after so bad a seed time, and severe a winter.

October was a wet and windy month, but that did not hurt; after so dry a summer. The wheat seed time

time was very fine; and the end of October, November, and part of December, were chiefly fair, fine and mild, ſaving much of people's fodder, which is ſcarce, and the ground is much drier than uſual at the time of year. (On the other hand, in ſome parts of Northumberland, Cumberland, and Durham, there were at this time ſome ſuch terrible rains, as made prodigious and deſtructive floods.) It continued open and mild to the end of the year; but part of December was wetter, making the ground dirty, yet not deep.

VI. *Directions*

VI. *Directions for using the common Micrometer, taken from a Paper in the late Dr.* Bradley's *Hand-writing; communicated by* Nevil Maskelyne, *Astronomer Royal, and F. R. S.*

Read Feb. 20, 1772.

MICROMETERS, as first contrived, being only adapted to the measuring small angles, as the diameters of the Sun and Moon, or other planets, and taking the distance of such objects as appeared within the aperture of the telescope at the same time, were not of so general use as those which are contrived not only to answer the ends that the first inventers aimed at, but likewise, to take the difference of right ascension and declination of such objects as are farther asunder than the telescope will take in at once, or that which pass through the aperture of it at different times. Mr. Cassini first made use of threads intersecting one another at half right angles for determining the difference of right ascensions and declinations of objects near the same parallel; and this apparatus being simple and easily procured is of very great use to such as are not provided with a micrometer made according to the late improvements. But, where such a one is at hand, that method however curious need not be made use of, the micrometer

meter ſerving for the ſame purpoſe with greater exactneſs. It was for this reaſon indeed that the late alteration in the form of the micrometer was made, they being before not ſo convenient for making ſuch ſort of obſervations, both hairs being uſually moveable, and no proviſion being made for ſetting the hairs parallel to the diurnal motion of the objects to be obſerved; both which inconveniencies are avoided in the preſent micrometers.

The micrometer, as now contrived, is not only of uſe in meaſuring ſmall angles or diſtances between ſuch objects as appear within the aperture of the teleſcope at the ſame time, but likewiſe in taking the difference of right aſcenſion and declination between ſtars and planets, &c. which in their apparent diurnal motion follow one another through the teleſcope if kept in the ſame ſituation. In making the firſt kind of obſervations, turn the ſhort tube which carries the eye glaſs and micrometer, &c. till the croſs thread (or that which cuts the parallel threads at right angles) lies parallel to a line paſſing through the objects whoſe diſtance is to be meaſured, and then by raiſing or depreſſing the teleſcope by help of the ſtand bring the objects to appear upon or near the croſs thread, and one of them juſt to touch the fixt parallel thread: then turn the index of the micrometer till the moveable thread touches the other object, and the number of revolutions and parts of a revolution ſhewn by the index, turned into minutes and ſeconds by the table made as hereafter directed, will be the apparent angular diſtance of thoſe objects. It is here ſuppoſed, that the threads exactly cloſe, ſo as to touch each other

when

when the index ſtands at the beginning of the diviſions: for, if they do not, there muſt be an allowance made in every obſervation; to avoid which, it is always beſt to adjuſt the threads to the beginning of the diviſions when they are firſt put on; for which purpoſe the holes in the little plate which carries the moveable thread are made oblong to give room to move it as occaſion requires, before it is pinched hard by the ſmall ſcrews which faſten it to the moveable arm, through which the long ſcrew paſſes. The other parallel thread, which I call the fixed one, muſt be firſt adjuſted by ſetting its edge exactly over the two marks made on each ſide the ſhort diameter of the aperture in the broad plates, and the croſs thread muſt be likewiſe ſet to agree with the ſtrokes made on each ſide the longeſt diameter, and then the interſection of the croſs thread and the fixt parallel one will be the center of the motion given to the outer plate of the micrometer (to which the great ſcrew index and threads are faſtened) by the worm, by turning of which the fixt parallel thread may eaſily be made to lie parallel to the apparent motion of any object in order to take the difference of declination and right aſcenſion from any other that follows through the aperture of the teleſcope.

This contrivance is of very great uſe to make a ſtar, &c. move true along the fixt parallel thread, which is abſolutely neceſſary in order to take the true difference of right aſcenſion and declination between it and any other that follows. Without this contrivance it is very difficult to make a ſtar move exactly upon the thread, and it can only be done by repeated

peated trials, which may ſometimes take up a great deal of time.

If therefore a ſtar is made to move on the parallel thread juſt at the croſs, and (the teleſcope continuing ſixt in the ſame poſition) it is afterwards near its going out of the aperture found not to be upon the thread, that muſt then be brought to the ſtar by the help of the worm, and then the thread will lie parallel to the diurnal motion of the ſtar in that part of the heavens, and conſequently the croſs thread will repreſent a meridian, and the others parallels of declinations, and the difference of time between the paſſage of the ſtar at the croſs wire (which was made to move along the thread), and the tranſit of any other ſtar, &c. over the croſs thread which repreſents a meridian, turned into degrees and minutes, will give the difference of right aſcenſion And, if the moveable parallel thread be brought, by turning the index, to touch the other ſtar about the time of its paſſage over the croſs thread, then the number of revolutions and parts ſhewn by the index (turned into minutes and ſeconds of a degree by the table) will be the difference of declination between the two ſtars. If the ſtar is made to paſs along the fixed thread ſo as to ſeem perfectly biſected, there muſt be an allowance made for the ſemidiameter of the thread or wire, becauſe I ſuppoſe the index to be adjuſted as before to the inner edges of the wires; but it may, if it is found convenient, be adjuſted to the middle of the threads, or elſe correction may be made in the obſerved diſtance.

In taking any angle, it is convenient that each of the parallel threads be about the ſame diſtance from

 the

the middle of the aperture of the eye-glafs; and for this reafon the whole micrometer is contrived to flide to and fro, as the cafe requires. The fame motion is alfo of ufe in taking the difference of right afcenfion and declination, by fliding the fixt parallel thread (on which the preceding ftar is brought to move) towards one fide of the eye-glafs; for by that means a greater angle may be taken in between the parallel threads, if need be. And it muft always be remembered that the moveable parallel thread fhould be fet either north or fouth of the other, according as the following ftar is expected to be really fouth or north of the preceding.

In making an obfervation, either the inner or the outer edges or the middle of the wires may be brought to touch the objects; but then, it muft be remembered to allow fomething for the thicknefs of the wire, in cafe the obfervation be not made from that part to which the index is adjufted. In obferving the diameters of the fun moon or planets, it may perhaps be moft convenient to make ufe of the outer edges of the threads, becaufe they will appear moft diftinct when quite within the limb of the planet; &c. but if there fhould be any fenfible inflection of the rays of light in paffing by the wires, this would be beft avoided by ufing the inner edge of one wire and the outer edge of the other. And in taking the diftance or difference of declination between two ftars, &c. the middle of the threads may perhaps be moft convenient: but, however the obfervation is made, due correction muft be allowed for the thicknefs of the wire, if requifite.

The

The difference of declination of two ſtars, &c. may be obſerved with great exactneſs, becauſe the motion of the ſtars is parallel to the threads; but in taking any other diſtance, the motion of the ſtars being oblique to them is a great impediment, becauſe if one ſtar be brought to one thread before the eye can be directed ſo as to judge how the other thread agrees to the other ſtar, the former muſt be ſomewhat removed from its thread, ſo that in this ſort of obſervations the beſt way of judging when the threads are at the proper diſtance is by frequently moving the eye backwards and forwards from one to the other: this method muſt chiefly be made uſe of when the diſtance of the objects is pretty large, and the motion or rowling of the eye great.

The micrometer is ſo contrived that it may be applied to teleſcopes of different lengths; but then, there muſt be a table for each teleſcope, by which the revolutions of the ſcrew may be turned into minutes and ſeconds of a degree. In order to this it is neceſſary that the threads of the micrometer ſhould be placed exactly in the common focus of the object-glaſs and eye-glaſs, that is, where the images of objects ſeen through the teleſcope are diſtinctly formed. The readieſt way of doing this is, firſt to ſlide the micrometer into the grooves fixt to the ſhort braſs tube, which carries the whole apparatus of eye-glaſs, &c. and then to draw the eye-glaſs out by means of its ſliding work, till the threads of the micrometer are in its focus, which is known by their appearing moſt diſtinct, &c. Then thruſt the ſhort tube before-mentioned into its proper place, as far

 as

as the ſhoulders of the braſs work will admit, and place the object glaſs in its cell, and looking through the teleſcope at ſome very diſtant object ſlide the wooden tube in or out till you make the object appear moſt diſtinct, or till it has the leaſt motion upon the threads when the eye is moved to and fro; for then the threads of the micrometer will be in the common focus of both glaſſes, and that will be the proper diſtance that the object-glaſs ought always to be at from the threads; and there ſhould be made ſome mark or ketch in the wooden tube in order to ſet it always at the ſame diſtance.

The proper diſtance of the threads from the object-glaſs being thus ſettled, the table for turning the revolutions, &c. of the ſcrew into angles or minutes and ſeconds of a degree may be made ſeveral ways; but as good and eaſy a method as any is carefully to meaſure how many inches and parts of an inch the object-glaſs is diſtant from the threads, and with the ſame ſcale to find alſo how many inches and parts of an inch an hundred, &c. revolutions or threads of the ſcrew of the micrometer are equal to: then, making the firſt diſtance radius, the laſt will be the ſine or tangent of an angle anſwering to 100 revolutions. And having the angle anſwering to 100 revolutions, the angle for any other number will be eaſily known and ſet down in the table, as alſo the parts of a revolution; for in ſmall angles, ſuch as can be obſerved with the micrometer, their ſines tangents or cords are nearly in the ſame proportion with the angles themſelves. The diſtance before-mentioned (to be uſed as radius) ought ſtrictly to be taken from the threads to a

point

point within the object-glaſs about one third of its thickneſs from that ſurface which is towards the wires, if the glaſs be, as uſual, equally convex on both ſides; but if the focus of the object-glaſs is pretty long and its thickneſs not great, the error that can ariſe by meaſuring from any part of the object-glaſs will become inſenſible as to the alteration in the angle.

The table for the micrometer may likewiſe be made by ſetting up two marks at a diſtance on the ground, and obſerving with the micrometer the revolutions, &c. which they ſubtend when ſeen through the teleſcope, and then computing the angles thoſe objects ſubtend at the object-glaſs, by meaſuring their diſtance from each other and from the object-glaſs. The like may alſo be done by opening the threads to any number of revolutions, and then making a ſtar move exactly upon the perpendicular thread, and noting the time it is paſſing from one parallel thread to the other; for that time turned into minutes and ſeconds of a degree, by allowing for the ſtar's declination and going of the clock, &c. will be the angle anſwering to the number of revolutions; from which the whole table may be made. This method perhaps might be moſt advantageouſly practiſed in ſtars' near the pole, where the apparent motion being ſlow a ſecond in time will anſwer to a much ſmaller angle than towards the equator. But I believe, upon trial, the firſt method will be found moſt eaſy and practicable, eſpecially if the ſcale made uſe of be well divided.

VII. *A Letter from Mr.* John Reinhold Forster, *F. R. S. to* William Watson, *M. D. giving some Account of the Roots used by the* Indians, *in the Neighbourhood of* Hudson's-Bay, *to dye Porcupine Quills.*

N° 2, Somerset Stable-yard, Strand,
Jan. 16, 1772.

SIR,

Read Feb. 27, 1772.

AMONG the curiosities presented by the Hudson's Bay Company to the Royal Society, is a small parcel of porcupine quills, dyed by the wild natives, some red and some yellow, together with the roots of some plants they use for that purpose.

I examined them carefully, at your desire, and found that they are probably of the same kind with those mentioned by Prof. Kalm, vol. iii. p. 14. and 160 of the English translation. The one root, dying yellow, is called by the French in Canada, Tissavoyanne jaune; the other, dying red, has the name of Tissavoyanne rouge. Prof. Kalm declares the latter to be a new plant, belonging to the genus of Galium, and received by Dr. Linnæus in his Species Plantrum, p. 153. by the specific name of Tinctorium,

on account of its dying quality. It grows in woody, moiſt places, in a fine ſoil. Kalm obſerves, " that " the roots of this plant are employed by the In- " dians in dying the quills of the American Por- " cupine red, which they put into ſeveral places of " their work: air, ſun, and water, ſeldom change " this colour. The French women in Canada " ſometimes dye their cloth red with theſe roots, " which are but ſmall, like thoſe of the Galium " luteum or yellow bedſtraw."

Dr. Linnæus deſcribes this plant, as having ſix narrow linear leaves at each knot of the ſtem, and four at the branches; commonly two flowers are on each ſtalk, and its ſeeds are ſmooth. The roots, when dry, are of the thickneſs of a crow quill, brown on the outſide, and of a bright purple red, when broken, on the inſide.

The ſecond plant, or the Tiſavoyanne jaune, is according to Prof. Kalm, vol. iii. p. 160. " the " threeleaved Hellebore (*Helleborus trifolius* Linn.) " grows plentifully in woods, in moſſy, not too wet, " places. Its leaves and ſtalks are employed by the " Indians to dye yellow ſeveral kinds of their work, " made of prepared ſkins. The French learned " from them to dye wool and other things yellow " with this plant."

Among the roots ſent as a ſpecimen from Hudſon's-bay, I found ſeveral leaves, which I ſeparated, and found the plant undoubtedly to be the threeleaved Hellebore.

In the 4th vol. of Dr. Linnæus's Amœnitates Academicæ is a figure of this plant, which upon compariſon I found by no means to be accurate: for the

the leaves in our ſpecimens, and in thoſe collected by a gentleman who favored me with the ſight of the plant, are far more pointed, than in the engraved figure. The ſtalks have conſtantly but one flower.

The dyed porcupine-quills ſent along with the roots from Hudſon's-bay, are of the brighteſt red and yellow: and this circumſtance ſuggeſted to me the thoughts of trying whether theſe roots might not be uſefully employed in dying. I mentioned it to you, and was encouraged to make ſuch a trial, as the ſmall quantity of the roots would permit.

I boiled a piece of flannel in a ſolution of half ſalt of tartar and half alum: the wet flannel was hereupon put into the decoction of the threeleaved Hellebore-roots, and boiled in it for the ſpace of about 12 or 15 minutes; the flannel, when extracted, was dyed with a bright and laſting yellow dye. A white porcupine quill, boiled in the ſame decoction, became nearly of as bright a yellow, as thoſe ſent over from Hudſon's-bay. This experiment made me believe, that I had hit upon the right method of dying with the threeleaved Hellebore; and will, I hope, prompt the directors of the Hudſon's-bay Company to order larger quantities of this root from their ſettlements, as it will no doubt become an uſeful article of commerce.

The flannel, boiled in ſalt of tartar and alum as above-mentioned, was likewiſe immerſed and boiled for nearly the ſame ſpace of time as in the former experiment, in a decoction of the root of the Galium Tinctorium, but it would dye only a dull and faint red. A porcupine quill boiled with it became yellow, but by no means red. This operation

tion convinced me, that the Indians muſt certainly have ſome method or other to extract the bright and laſting colour, which I could not perform. They uſe perhaps the root quite freſh, which circumſtance probably makes them ſucceed in their dying proceſs. If it could be brought about, to extract and afterwards to fix on wool the dye of this root, it would, no doubt, on account of its bright colour, be a valuable acquiſition for our manufactures: and I do not in the leaſt doubt of the probability to ſucceed in the attempt, as the wollen ſtuffs are animal ſubſtances as well as the porcupine quills, and therefore eaſily ſuſceptible of any dye.

The directors of the Hudſon's-bay Company will, we hope, order their ſervants at the ſettlements to examine carefully and minutely, the method employed by the Indians in dying red with this root; and to ſend an account thereof, and greater quantities of this root over, that ſeveral chemiſts may be enabled to make experiments at large with them; for often, in dying, the experiments will not ſucceed, when tried in ſmall quantities.

The wild inhabitants of North America are certainly poſſeſſed of many important arts; which, when thoroughly known, would enable the Europeans to make a better, and more extenſive uſe of many unnoticed plants, and productions of this vaſt continent, both in phyſic, and in improving our manufactures, and erecting new branches of commerce.

To give an inſtance of this, I will only mention, that the Spaniards of Mexico have but lately learnt of the inhabitants of California, the art of dying the

the depeest and most lasting black, that ever was yet known. They call the plant they employ for that purpose Cascalote; it is arboreous, with small leaves and yellow flowers; its growth is still slower than that of an oak; it is the least corrosive of all the known substances employed in dying, and strikes the deepest black; so that, for instance, it penetrates a hat to such a degree, that the very rags of it are thoroughly black. The leaves of the Cascalote are similar to those of the Husiaoke, another plant likewise used for dying black with, but of an inferior quality. The latitude of California lets us hope, that the country near the Mississippi, or one of the Florida's, contains this Cascalote, the acquisition of which would be of infinite use in our manufactures.

Were Natural History thus employed in applying the natural productions for procuring the necessaries, or adding to the comforts and ornaments, of human life, it would for the future free this science from the vulgar opinion, that it is merely speculative, and incapable of being of the least utility in common life; a prejudice which gains more ground by the injudicious and unprofitable manner, now chiefly in vogue, in studying this branch of human knowledge; and which might be removed, if powerful trading companies would encourage the efforts of the naturalist, by enabling them to search the treasures of nature, in the various countries subject to the British Crown, and connected with its subjects by trade and commerce Pardon, Sir, that I detain you so long on a point of which you are so well convinced, and which

which you have frequent opportunities to convince others of. I am, with the trueſt regard,

SIR,

Your moſt obedient,

humble ſervant,

John Reinhold Forſter.

VIII. *An Account of a Subærated Denarius of the* Plætorian *Family, adorned with an* Etruſcan *Inſcription on the Reverſe, never before publiſhed or explained. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

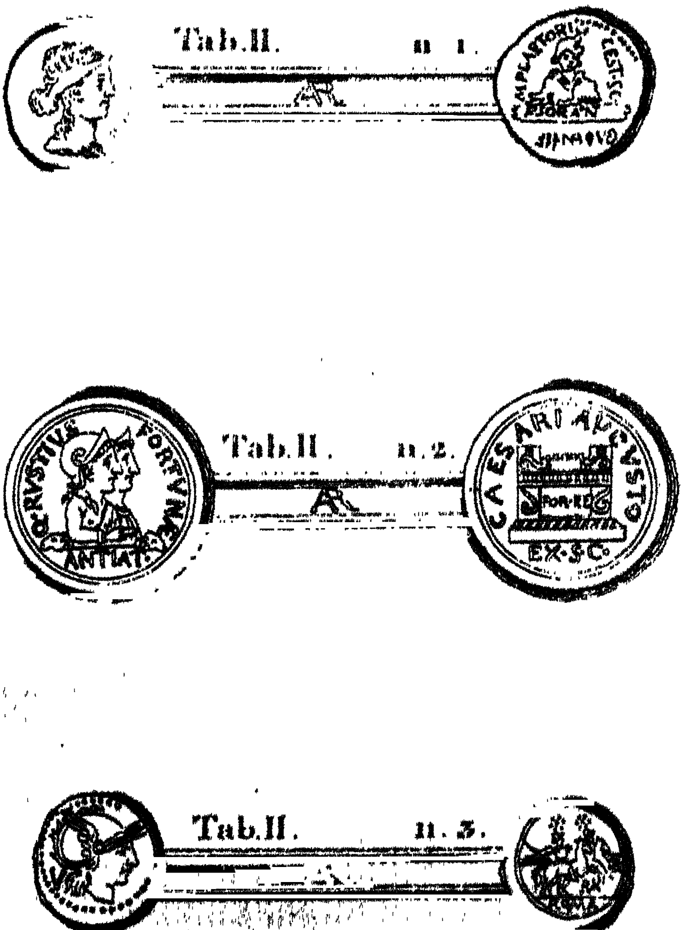
Dear Sir,

I.

Read Feb. 27, 1772.

THE piece I have undertaken to conſider here is a ſubærated (ſee TAB. II. n. 1.) denarius of the Plætorian family, which lately fell into my hands. It exhibits on one ſide a female head, repreſenting the goddeſs Libera, or Proſerpina, according to M. (1) Havercamp, before which ſtand the letters ΓCOSINI, P COSINI, very ill preſerved. On the reverſe, we diſcover a buſt of the goddeſs SORS, on a ſort of baſis, adorned with the inſcription F SOR ANT, or rather AN; under which, in the exergue, appear the Etruſcan letters ⅃⅃ꟼИꟼIVƎ, FIR,

(1) Sig. Havercamp. *Commentar. in Fam. Roman. Numiſm. Omn. &c.* p. 313, 314.


Tab. II. n. 1.
Tab. II. n. 2.
Q·RVSTIVS FORTVNAE
ANTIAT
CAESARI AVGVSTO
FOR·RE
EX·S·C
Tab. II. n. 3.

Basire sc.

or rather FVR, ANTIE, *i. e.* FORS, FORTVNA, or SORS, ANTII, or ANTIAT, equivalent to the Latin inſcription above it. The Etruſcan elements ſeem rather better preſerved than the Latin. The coin is, however, in but indifferent conſervation, though pretty much of the thin ſilver plate remains ſtill upon it. I muſt not omit obſerving, that it has never yet been communicated to the learned world.

II.

The ſymbol on the reverſe here is the ſame that occurs on the reverſes of two or three (2) other conſular coins of the Plætorian family, with the word SORS attending it. The Latin inſcription, on the piece before me, is extremely ſimilar to one upon a denarius of the Ruſtian family, now in my ſmall collection, a draught of which may be ſeen in the plate (3) here referred to. The ſymbol there is a double Fortune, or rather two galeated Fortunes, which were conſidered as deities by the Romans. The divinity SORS, on the coins of the Plætorian family, ſimilar to mine, is aſſerted by (4) Vaillant and Havercamp to be the SORS, or rather one of the SORTES, worſhiped in the temple of the SORTES at Præneſte; whereas it was the SORS, or rather one of the SORTES, adored in the temple of thoſe deities at Antium, as we find demonſtratively proved by the coin I am attempting to explain. The whole

(2) Sig. Haverc. in *Fam. Plætor.* Tab. I. n. 1, 2, 3.
(3) See TAB. II. n. 2.
(4) I. Vaill. *Num. Antiqu. Familiar. Romanar.* p. 238, 239, 240. Amſtelædami, 1703. Sig. Haverc. *Comment. &c.* p. 324.

ſuper-

superstructure, therefore, erected by those two learned men on that supposition must necessarily fall to the ground.

III.

The Etruscan inscription, on the reverse of my denarius, in the exergue, seems to allude to a passage in Tully, relative to the origin of those deities denominated SORTES by the Romans, and to be illustrated by, as well as to throw some light upon, that famous passage. As this point is extremely curious, I shall beg leave to transcribe the whole passage, which has been handed down to us (5) in the following terms: "Numerium Suffucium Prænestinorum monumenta "declarant, honestum hominem ac nobilem, somniis "crebris, ad extremum etiam minacibus, cum juberetur certo in loco, silicem cædere, perterritum visis, "irridentibus suis civibus, id agere cœpisse: itaque "perfracto saxo Sortes erupisse, in robore INSCVLPTIS "PRISCARUM LITERARUM NOTIS. Is est hodie "locus septus religiose propter Jovis pueri, qui lactens "cum Junone, Fortunæ in gremio sedens, mammam "adpetens, &c." "In some of the antient monuments of Præneste, mention is made of one Numerius Suffucius. This man, who was one of the "most considerable and most venerable persons in "his city, both for his probity and noble extraction, "was admonished, in different dreams, and at last "with terrible menaces, to go to a certain place in "Præneste, and there cut a flint. Being terrified

(5) Cic. *De Divinat.* Lib. II. c. 41.

" with these frequent visions, he obeyed. He came " to the place appointed, and there, in the presence " of several of his fellow-citizens, who laughed at his " attempt, tried to cut a flint; which, to the great " surprize of the spectators, gave way to the edge of " the knife. And out of the body of the flint drop- " ped several pieces of wood, (or rather oak) each of " which had an inscription IN THE ANTIENT " CHARACTERS. The place where this prodigy was " performed is now walled in, because in it is very " religiously kept an image of Fortune, holding Ju- " piter and Juno, represented as infants, in her arms, " &c." As I have formerly (6) proved that THE ANTIENT CHARACTERS OF ITALY, or the PRISCARUM LITERARUM NOTÆ of the Romans, were the Etruscan letters; and as the inscription formed of those characters, mentioned by Tully, in the passage here produced, cannot well be supposed (7) to have contained any other word than , FIR, or rather FVR, applicable to the deity, or deities, so called, and worshiped, both at Antium and Præneste; we may fairly suppose the Etruscan inscription before me to have glanced at the celebrated passage just pro-

(6) *De Priscis Romanorum Literis Dissertat.* Oxonii 1746. *Philosoph. Transact.* Vol. LXI. p. 88, 89.

(7) This must be allowed extremely probable, as the pretended origin of the LOTS, mentioned by Tully, must have been supposed prior to the foundation of their temples at Antium and Præneste, and therefore the original inscription was only, in all probability, supposed to have been either FIR, or FVR; though, after the erection of those temples, the deity or deities, now in view, might have been denominated F SOR ANT, and FOR ANT, on antient Roman coins. Vid. J. Vaill. & Sig. Haverc. in *Fam. Plætor.* et *Rust.*

duced,

duced, and conſequently that this paſſage and my explication of that inſcription may be preſumed mutually to ſupport and illuſtrate each other.

IV.

The firſt of the Etruſcan elements, on the reverſe of my coin, , is apparently that letter in the Etruſcan alphabet which, in power, is equivalent (8) to F, or PH, though the character here differs ſomewhat from all the forms of that element that have hitherto occurred to me on the Etruſcan monuments. The ſecond is either I, or, as I am more inclined to believe, V. That it ought rather to be conſidered as V, ſeems to me to appear from the obliquity of its poſition, in reſpect of the firſt letter; which ſeems to indicate the ſide of the V next to that letter to have been effaced, by the injuries of time. The third is undoubtedly the antient Tuſcan , (9) or R, ſomewhat blotted, or blurred. The fourth and fifth manifeſtly form the monogram , or AN, which has not yet occurred to me on any other Etruſcan* monument. The ſixth, ſeventh, and eighth, , are evidently equivalent

(8) Anton. Franciſc. Gor. *Muſ. Etruſc.* Vol. II. p. 416, 417. Florentiæ, 1737. (9) Id. ibid. p. 412, 417.

* From this inſtance, as well as others, that might eaſily be produced, it appears, that the Etruſcans ſometimes made uſe of monograms, as well as the Greeks, Romans, and Phœnicians. As the Romans, therefore, ſeem to have uſed monograms before the commencement of any intercourſe with the Greeks, as is rendered probable by the very antient inedited quinarius here referred to (ſee TAB. II. n. 3.), which was, as I conceive, ſtruck before the cloſe of the fifth century of Rome; I am inclined to believe, that they borrowed this manner of writing from the Etruſcans.

(10) to the Latin, or Roman, letters TIE. The whole inscription is therefore , FIR, or rather FVR, ANTIE, SORS, FORS, or FORTUNA, ANTII, altogether equipollent to the Latin inscription above it. That FVR ANTIE, in the antient Etruscan language, ought to be rendered, in Latin, FORS, SORS, or FORTVNA, ANTII, is apparent, from one of the Tables of Gubbio; which, according to (11) Sig. Olivieri, exhibits the words AGRE TLATIE, equivalent to AGRI LATII, and is directly in point. Hence we may conclude, that FIR, or rather FVR, answers to the FORS, or FORTVNA, of the Latins, the Etruscans using constantly V for O; and consequently that פור, PVR, PHVR, or FVR, in (12) Hebrew, or, as some will have it, in the (13) antient Persic, FIR, or FVR, in Etruscan, and FORS, or FORTVNA, in Latin, denoted the very same thing.

V.

That SORS, or SORTES, and FORTVNA, probably the same deity, (14) were worshiped both

(10) Anton. Francisc. Gor. ubi sup. p. 407, 409, 414, 417

(11) *Una Lettera del Signor* Annibale degli Abati Olivieri, *&c. Al Signor Abate* Barthelemy, *&c.* p. 42. In Pesaro, 1757.

(12) Esth. III. 7.

(13) Val. Schind. Lex Pentaglot. p. 1432. Hanoviæ, 1612.

(14) Hence we find the celebrated temple of the LOTS, or SORTES, at Præneste to have been also denominated the temple of FORTUNE; nor do I doubt but the famous temple of the LOTS or SORTES, at Antium went likewise under that denomination. Sig. Havercamp. ubi sup. p. 324, 325.

 by

by the Romans and the Etrufcans, will (15) not admit of a doubt. The Romans feem to have ufed the words SORS and FORTUNA for one deity, on fome occafions; and, on others, the term SORTES, as applicable to more divinities, and FORTVNAE, or FORTUNAE ANTIAT, as relative to two, to whom they affigned the (16) epithets FORTIS, and FELIX. One or both of thofe epithets may poffibly be pointed out to us by the letter F, which precedes the words SOR ANT, on the bafis below the buft of the goddefs SORS, on the reverfe of the coin in queftion. But that this is the true import of the word to which that letter belongs, I muft by no means take upon me pofitively to affirm.

VI.

The medals of the Plætorian family fimilar to that I have been confidering Havercamp (17) takes to have been ftruck in the time of the civil war, that fucceeded Julius Cæfar's death; in which, perhaps, he may not be very remote from truth, though this he has not irrefragably proved. If it fhould, however, be allowed probable by the learned, the coin before me, which muft be nearly of the fame date with that war, will feem to have preceded about forty years the birth of CHRIST.

(15) Vid. Anton. Francifc. Gor. ubi fup. p. 214. 215. Sam. Pitifc. in *Lex. Antiquitat. Romanar.* paff. aliofque author. quam plurim.

(16) Fabrett. *Infcript. Antiqu.* cap. ix. p. 632. Sig. Haverc. ubi fup. p. 324, 369. Amftelædami, 1734. Vid. etiam Vulp. *Vet. Lat. Profan.* tom. III. cap. v. p. 98, & feqq.

(17) Sig. Haverc. ubi fup. p. 325.

VII.

VII.

Who P. Cosinius, whose name seems to have been handed down to us by the Denarius I have been attempting here to explain, was, or what was the particular mode of his connection with M. Plætorius, by whom the piece was struck, I cannot at present, for want of sufficient light from antient history, and authentic Roman monuments, take upon me to decide. But this I may be allowed to say, that the piece before me is the only coin of the Cosinian family that has hitherto escaped the ravages of time. That the Cosinian family was of some note in Rome, we may infer, not only from the very curious denarius that is the object of my attention here, but likewise from two or three antient (18) Roman inscriptions, which have preserved to us the name of that family. As for M. Plætorius, mentioned on the denarius before me, (19) and other similar coins, he was, according to M. Havercamp, (20) questor to Brutus, one of Cæsar's murderers; and the piece I am endeavouring to explain first appeared, as already observed, a little (21) after that emperor's death. The Etruscan letters were not then intirely out of use: nay, they were not totally disused in some parts of Italy, and particularly at (22) Falerii, a considerable

(18) Jan. Gruter. *Corp. Inscript. ex recens. Jo. Georg. Græv.* p. CMLXXI. 9. DCLVIII. 1. Amstelædami, 1707. Ludovic. Anton. Murator. *Nov. Thesaur. Vet. Inscript.* p. DCCXCIV. 7. Mediolani, 1740.

(19) Vid. I. Vaill. & Sig. Haverc. in *Fam. Plætor.*

(20) Sig. Haverc. ubi sup. p. 325.

(21) Id. Ibid.

(22) Strab. *Geogr.* Lib. V.

number of years after that tragical event. This we learn from Strabo, who flouriſhed when Tiberius ſat upon the imperial throne.

VIII.

Having now finiſhed my attempt to elucidate a very curious inedited Etruſcan coin, highly meriting the attention of the learned; I would flatter myſelf, that an acquiſition is hereby made to the ſcience of antient medals, and conſequently that this paper may prove not altogether unacceptable to the Royal Society. You will therefore be pleaſed to lay it before that very learned and moſt illuſtrious body; and believe me to be, with all poſſible conſideration and eſteem,

SIR,

Your much obliged,

and very affectionate,

humble Servant,

Chriſt-Church, Oxon.
Oct. 10, 1771.

John Swinton.

IX. *A Deduction of the Quantity of the Sun's Parallax from the Comparison of the several Observations of the late Transit of* Venus, *made in* Europe, *with those made in* George Island *in the* South Seas: *Communicated by Mr.* Euler, *jun. Secretary of the Imperial Academy of* Petersburg; *in a Letter to* Charles Morton, *M. D. Sec. R. S. and Acad. Imper.* Petroburg, *&c. Soc.*

Read March 5, 1772.

OBSERVATIONES noviſſimi tranſitus Veneris ante diſcum Solis, A. 1769, in inſula maris Auſtralis, King George Iſland dicta inſtitutas, calculo nuper ſubjecit academicus noſter D. Lexell, earumque comparationem cum obſervationibus celebratiſſimi hujus phænomeni in Europa factis inſtituit, ut quantitatem parallaxis Solaris erueret. Longum omnino foret, ſi omnium calculorum ab ipſo inſtitutorum juſtam heic adferre vellemus expoſitionem, idemque etiam minus neceſſarium, quum diſſertatio ejus hac de re conſcripta, Tomo XVI. noſtrorum Commentariorum inſerenda ſit; ſumma igitur tantum capita concluſionum inventarum perſtringere ſuffecerit. In anteceſſum vero e re eſt monuiſſe, methodum ab ipſo adhibitam eandem eſſe, quam Illuſtr. Eulerus invenit et quæ in

part.

part. II. Tom. XIV. nov. Comment. ſuſe explicata fuit.

Siquidem obſervationes contactuum externorum ſuper inſula King George inſtitutæ, cum momentis pro contactibus internis allatis, non ſatis bene conſentire videantur, auctor noſter duos caſus ſeparatim ſibi conſiderandos eſſe ratus eſt, priorem quo ratio habetur tam contactuum externorum, quam internorum, alterum vero, quo contactus tantum interni adhibentur. Combinatione autem facta obſervationum inſulæ King George cum iis in Europa inſtitutis, pro priori caſu inventa eſt parallaxis Solis horizontalis $\pi = 8{,}68 - 0{,}0077\, y$, pro poſteriori $\pi = 8{,}58 - 0{,}0080\, y$; ubi notandum eſt y ſignificare correctionem latitudinis geocentricæ Veneris pro aſſumto tempore conjunctionis. Dum ſimili ratione obſervationes in Fortalitio Principis Walliæ ad Sinum Hudſonis factæ, cum Europeis comparantur, habetur pro priori caſu $\pi = 8{,}82 - 0{,}0019\, y$ et pro poſteriori $\pi = 8{,}74 - 0{,}0022\, y$. Obſervationes denique Californienſes, cum Europeis comparatæ præbent $\pi = 8{,}61 - 0{,}0062\, y$. Ut inter has concluſiones medium quoddam ad veritatem proxime accedens eligi poſſet, notandum omnino fuit, ſingulis earum eo majorem certitudinis gradum tribui debere, quanto majores fuerint coefficientes, quibus litera π in illis aequationibus affecta deprehenditur, ex quibus valores ſupra allati hujus quantitatis eliciti ſunt; quo major enim hujuſmodi coefficiens fuerit, eo ſane minorem influxum errores in obſervando commiſſi habebunt, ad verum valorem Parallaxis immutandum. Probabilitates igitur concluſionum ex ſingulis obſervationibus Americanis deductarum hoc modo æſtimando,

inventum

inventum eſt, eas reſpective habendas eſſe proportionales numeris 11, 8, et 4. Porro ut obſervationes majori fide dignæ ab incertioribus ſecerni poſſent, auctor noſter tres ſtatuit hypotheſes; 1. qua modo ſupra dicto medium ſumitur ex concluſionibus quæ inveniuntur, dum omnes ſine diſcrimine obſervationes in uſum vocantur et pro qua habetur $\pi = 8,63 - 0,0063\,y$; 2. qua pro inſula Regis Georgii momenta ſolum contactuum internorum in computum ducuntur, unde deducitur $\pi = 8,57 - 0,0057\,y$, et 3. denique qua obſervationes contactuum externorum ad Sinum Hudſonis factæ excluduntur, quæ præbet $\pi = 8,62 - 0,0065\,y$. Quum tamen nulla ſufficiens adeſſe videatur ratio cur momenta contactuum externorum ad Sinum Hudſonis pro dubiis haberi deberent, medio quaſi ſumto, inter media ex binis poſterioribus hypotheſibus deducta parallaxis tuto ſtatui poſſe videtur $\pi = 8,60 - 0,006\,y$. Ad hujus concluſionis ulteriorem verificationem, ſingulæ obſervationes Americanæ comparatæ fuerunt cum iis in Lapponia factis, ubi tam ingreſſum quam egreſſum Veneris obſervare licuit, ſiquidem pro hujuſmodi obſervationibus, errores qui ex longitudinibus locorum perperam æſtimatis in parallaxin redundant fere nullius ſunt momenti. Tum autem mediis uti ſupra dictum eſt captis, inventi ſunt pro ſingulis tribus hypotheſibus ſequentes valores ipſius π:

1. $\pi = 8,68 - 0,0076\,y$. 2. $\pi = 8,67 - 0,0074\,y$. 3. $\pi = 8,62 - 0,0077\,y$. Hæ autem concluſiones a ſupra inventis non magis diſcrepant, quam ut diverſitatis ratio ex leviuſculis obſervationum erroribus facile reddi queat. Ex ſingulis denique obſervationibus Americanis inter ſe collatis ſequentes eliciti ſunt

valores

valores ipſius π; ex contactibus externis ad Sinum Hudſonis et ſuper inſula King George $\pi = 9,16 - 0,011\, y$; ex contactibus internis in iiſdem locis $\pi = 8,47 - 0,011\, y$; ex contactibus internis ad Sinum Hudſonis et Californiæ $\pi = 8,46 - 0,0096\, y$; ex contactibus denique internis Californiæ et ſuper Regis Georgii inſula $\pi = 8,48 - 0,0012\, y$.

Quod concluſionem ex contactibus externis deductam attinet, facile quidem liquet, eam plus juſto a veritate aberrare; quod vero determinationes ex contactibus internis derivatæ, quaſi in alteram partem peccent parallaxin ſupra inventa aliquanto minorem exhibentes, ex erroribus obſervationum facile admittendis provenire potuit. Ad valorem abſolutum ipſius π aſſignandum, quum jam requiratur, ut vera magnitudo correctionis y innoteſcat, ea ſollicite quoque determinanda fuit; momenta autem contactuum internorum in eo conſentire videntur, quod hæc correctio circiter 8″ ſtatui debeat, poſita ſemidiametro Solis 946,″38, quæ media eſt, inter ſemidiametri valorem a Cel. de la Lande aſſumptum, et quem aſtronomi Angli adhibere ſolent. Hoc autem valore pro y adhibito, erit parallaxis $\pi = 8,55$ ſec. ſemidiameter autem Veneris $= 28,6$ ſec. quæ ultima determinatio ultra 2 aut 3 partes decimas unius ſecundi erronea eſſe nequit, obſervationibus micrometro objectivo captis eam egregie confirmantibus. Si correctio latitudinis aliquanto eſſet minor, quod ex obſervationibus micrometricis redditur probabile, quum tamen infra 5″ certe deprimi nequeat, parallaxis inde ultra partem quinquageſimam ſecundi non reddetur dubia.

Elementa

Elementa autem astronomica ex valore parallaxis deducta jam ita se habebunt: conjunctio vera Solis et Veneris geocentrica contigit 1769 d. 3 Jun.

	10^h	$2'$	$32''$	Temp. medio Grenovicensi
seu	10	4	45	Temp. vero.

Pro quo momento erat,	s	°	′	″
1. Longitudo Solis et Veneris	2	13	27	20
2. Latitudo geoc. Veneris			10	18,8 Bor.
3. Parallaxis Solis horizontalis				8,55

Hinc facile elicitur longitudinem geographicam loci super insula King George, ubi observatio peracta fuit, a meridiano Grenovicensi statui debere $9^h\ 58'\ 6''$.

Quum pro insula Regis Georgii ea contactuum momenta, in calculo adhibita fuerint, quæ a Cel. Green assignata deprehenduntur, excepto momento contactus interni pro ingressu, ubi momentum a Cel. Doct. Solander notatum, in usum fuit vocatum, merito dispiciendum erat, quam subeat parallaxis mutationem, si alia contactuum momenta pro hoc loco notata adhibita fuissent. Quod igitur contactus internos attinet, duas supponere licuit hypotheses bina quasi extrema in se continentes, priorem qua observatio contactus interni pro ingressu a Cel. Green facta cum ea contactus interni pro egressu a Cl. Cook instituta combinatur, posteriorem qua mutata vice observatio prioris contactus interni a Dno Cook facta combinatur cum momento posterioris contactus interni a Cel. Green assignato. Prior hypothesis dat parallaxin $\pi = 8,48 - 0,0080\, y$, posterior vero $\pi = 8,65 - 0,0080\, y$, ubi medium $\pi = 8,57 - 0,0080\, y$

 vix

vix differt a determinatione ante adhibita. De momentis contactuum externorum generatim quidem liquet, ea cum obſervationibus internorum nequaquam conciliari poſſe, quoniam mora inter utriuſque generis contactus vera multo minor obſervata ſit; quo certius tamen conſtaret præcipuam aberrationis cauſam in contactibus externis eſſe quærendam, ſequenti modo in exactitudinem momentorum tam contactibus externis quam internis reſpondentium, auctori noſtro inquirere viſum eſt. Momenta contactuum internorum, cum obſervatione ejuſdem contactus pro ingreſſu Grenovici a Celeb. Maſkelyne facta comparat, indeque deducit aſſumtam longitudinem pro King George Iſland 22″ vel 25″ eſſe augendam: hæc deinde momenta ſimiliter comparando cum obſervatione contactus interni pro egreſſu in Gurjef a Cel. Lowits inſtituta, invenit eandem longitudinem 25 vel 20″ eſſe augendam, ex quo quum hæc correctio per quatuor diverſi generis comparationes fere eadem prodeat, concludendi rationem habere ſibi viſus eſt, in contactibus internis graves errores latere nequaquam probabile eſſe. Dum pro contactibus externis ſimile inſtituendum fuit examen, ne videretur parallaxin nimis parvam ſuppoſuiſſe, ſtudio maximam elegit quæ ex his contactibus deduci poteſt, ſcilicet $\pi = 9{,}04 - 0{,}0073\, y$, unde deducit correctionem longitudinis geographicæ pro King George Iſland ad 32″ aſſurgere. Poſtmodum facta combinatione binorum contactuum externorum, cum obſervatione contactus interni pro egreſſu in Wardhus a Rev. Pat. Hell inſtituta, invenit correctionem longitudinis per unam comparationem prodire 49 ſec. per alteram vero 16, quæ inſignis diſcrepantia certe non niſi ab incertitudine

dine contactuum externorum pro insula King George oriri potest, siquidem longitudo Wardhusii vix ultra $5''$ esse possit dubia.

Quoniam plurimis astronomis placuit parallaxin multo majorem adhibere, quam quæ nunc inventa fuit, operæ etiam pretium erat disquirere, quousque errores observationum pro contactibus internis pertingere debeant, si parallaxis supponatur vel 9 vel 10 secundorum. Inventum autem fuit pro parallaxi $9''$, si bini contactus interni pro Insula Regis Georgii, cum ejusdem nominis Europeis conferantur, summam errorum ad 40 fere secunda increscere, pro parallaxi autem 10 sec. eam duobus minutis primis minorem supponi non posse, quorum ut posterius absurdum, ita prius quoque valde improbabile videtur.

Deinde expendendum quoque fuit, utrum ex observationibus distantiarum minimarum, certi quicquam de parallaxi Solis concludi possit. Si autem comparatio instituatur distantiæ minimæ pro Insula King George inventæ cum distantis ad sinum Hudsonis, Noritoni in Pensylvania, et Novæ Angliæ a Cl. Winthrop mensuratis, inveniuntur pro parallaxi hi valores $\pi = 10'',51$, vel $\pi = 8'',98$, vel $\pi = 9'',97$, quorum dissensus jam certissimo est indicio quam parum fidei hujusmodi observationibus tribui debeat, in quæstione hujus generis decidenda. Cæterum si fuerint, qui ex his mensuris parallaxin $9''$ vel etiam 10 sec. deducere velint, ab illis sequentis dubii plenam solutionem merito expectamus: qui fiat, ut potius supponi debeat, singulis observationibus contactuum internorum a tot observatoribus institutis, ad minimum errores 20 sec. vel 30 sec. inesse, quam binas

vel tres menſuras diſtantiarum minimarum micrometro captas 2 aut 3 ſecundorum erroribus obnoxias eſſe? Denique et auctori noſtro placuit quædam adjicere de effectu atmoſphæræ Veneris ad durationem tranſitus imminuendam; obſervavit autem hanc quæſtionem in dubio relinquendam eſſe, donec exactiſſime determinare licuerit, tam latitudinem geocentricam Veneris, ſeu diſtantiam minimam veram, quam accuratam menſuram diametri Solis.

Præterea reticendum quoque non eſt, eundem D. Lexell obſervationes ſuper diſtantias marginum Solis et Veneris minimas, Noritoni inſtitutas, quas illuſtr. Societas Scientiarum nobiſcum communicavit, calculo ſubjeciſſe, et ex pulcherrimo conſenſu ſingularum fere obſervationum, concluſiſſe hanc diſtantiam minimam fore 10′ 10″ poſita ſemi-diametro Solis 947″, ſeu etiam 10′ 9″ poſita ſemi-diametro Solis 945″,5 uti celeb. Maſkelyne eam aſſignare ſolet. Exiſtimat autem idem D. Lexell utramque diſtantiæ minimæ quantitatem uno ſecundo augeri poſſe, quia probabilius videtur diſtantias marginum juſto majores eſſe captas, quam quod in defectu peccaverint.

X. *A Letter to the Rev. Mr.* Maſkelyne, *Aſtronomer Royal, F. R. S. accompanying a new Chart of the* Red Sea, *with two Draughts of the Roads of* Mocha *and* Judda, *and ſeveral Obſervations made during a Voyage on that Sea, by Capt.* Charles Newland.

S I R,

Read March 12, 1772.

I Beg leave to lay before you a chart of the Red Sea, conſtructed from materials that I became poſſeſſed of, during my reſidence in the Eaſt Indias; which chart, upon my voyage to Mocha and Judda, I experienced to be the beſt I have ever ſeen. The only material error that I ever diſcovered in it, is, that the Abyſſinian ſhore oppoſite Mocha is placed too far to the weſtward by 25 or 30 miles, and that there are ſeveral ſmall iſlands upon the ſame ſhore, not taken notice of in any other chart; which iſlands I have marked in the chart I now ſend you (See Tab. III.) together with two draughts of the roads of Mocha and Judda (Tab. IV.), which, if you think will be of any utility, are entirely at your ſervice.

I am, S I R,

With the utmoſt reſpect,

your moſt obedient humble ſervant,

Chas. Newland.

Longitude

Longitude of Judda by 12 Diſtances of the ☽ from ☉.

	°	′	″
Work'd by the Britiſh Mariner's Guide	39	53	45
And by the Ephemeris for 1769	40	1	7
Difference		7	22

	°	′	″	
By Jupiter's Satellites	39	26	45	E.
By ☽ and ☉	40	1	7	E.
		34	22	

XI. Remarks

XI. *Remarks and Observations made on board the Ship* Kelsall, *on a Voyage to* Judda *and* Mocha, *in* 1769, *by* the Same.

Read March 12, 1772.

IN my run from Socatra to Cape Aden, I made the dist. 8° 20′ W. and from Cochin 29° 39′ W. The latitude of the above Cape is laid down in most books and charts in 13° N. which I find is about 15 miles too much to the northward, according to the observation I took on the 10th of February 1769, as well as three other very good observers: by the medium of the whole we made the latitude 12° 41′ N. the Cape then bore N. N. W. ½ W. dist. 5 miles which gives near 4′ of latitude; that, added to 12° 41′ N. gives 12° 45′ N. for the latitude of the southernmost point of the Cape.

This cape, or headland, is one of the most remarkable I ever saw, when coming from the eastward; it is so very high and rugged, that it may be seen, I believe, 15 leagues at least, in fine weather. The tops of those ragged rocks resemble so many chimneys and spires; and, as you approach the cape, you see a zigzag wall, or whitish pathway, cut through the rocks, not at a very great distance from the waterside; a little below this, at the S. E. end,

end, you will ſee ſomething that looks very like two moſques; but this cannot be ſeen at a greater diſtance than 4 or 5 leagues; but when it is, you may be certain it is Cape Aden, and may then ſteer your courſe for Babelmandel accordingly.

A little to the weſtward of this cape, there is another high craggy headland, equally high and craggy as that of Aden, between which two there is an opening, very much reſembling a ſmall narrow ſtreight, but in reality it is only a deep bay, the bottom of which is very low land, ſo low, that it cannot be ſeen from the maſt-head, except you are cloſe in ſhore: by this deception, people have miſtaken it for the Streight of Babelmandel, and have been ſo far embayed, before they perceived their miſtake, that it was with the greateſt difficulty they got out again.

On each ſide of this bay lies a large rock, juſt at the entrance, and at about a quarter of a mile from the ſhore: when theſe are ſeen, you may be ſure it is not the Streight of Babelmandel. Was a ſhip to fall in with this place, and had not had an obſervation for ſome days before, I think it would be very eaſy to miſtake one for the other; there is only this difference, that Cape Aden is high and rugged, and Babelmandel is rather low and ſmooth, and the iſland (as the Directory obſerves) makes like a gunner's coin.

The beſt courſe to ſteer from Cape Aden to Saint Anthony is W. by S. by the compaſs, and that will carry you clear of the ſhoal lying off that point. I made the diſtance between Cape Aden and Cape Saint Anthony, by the ſhip's run, 17 leagues; the latter

cape

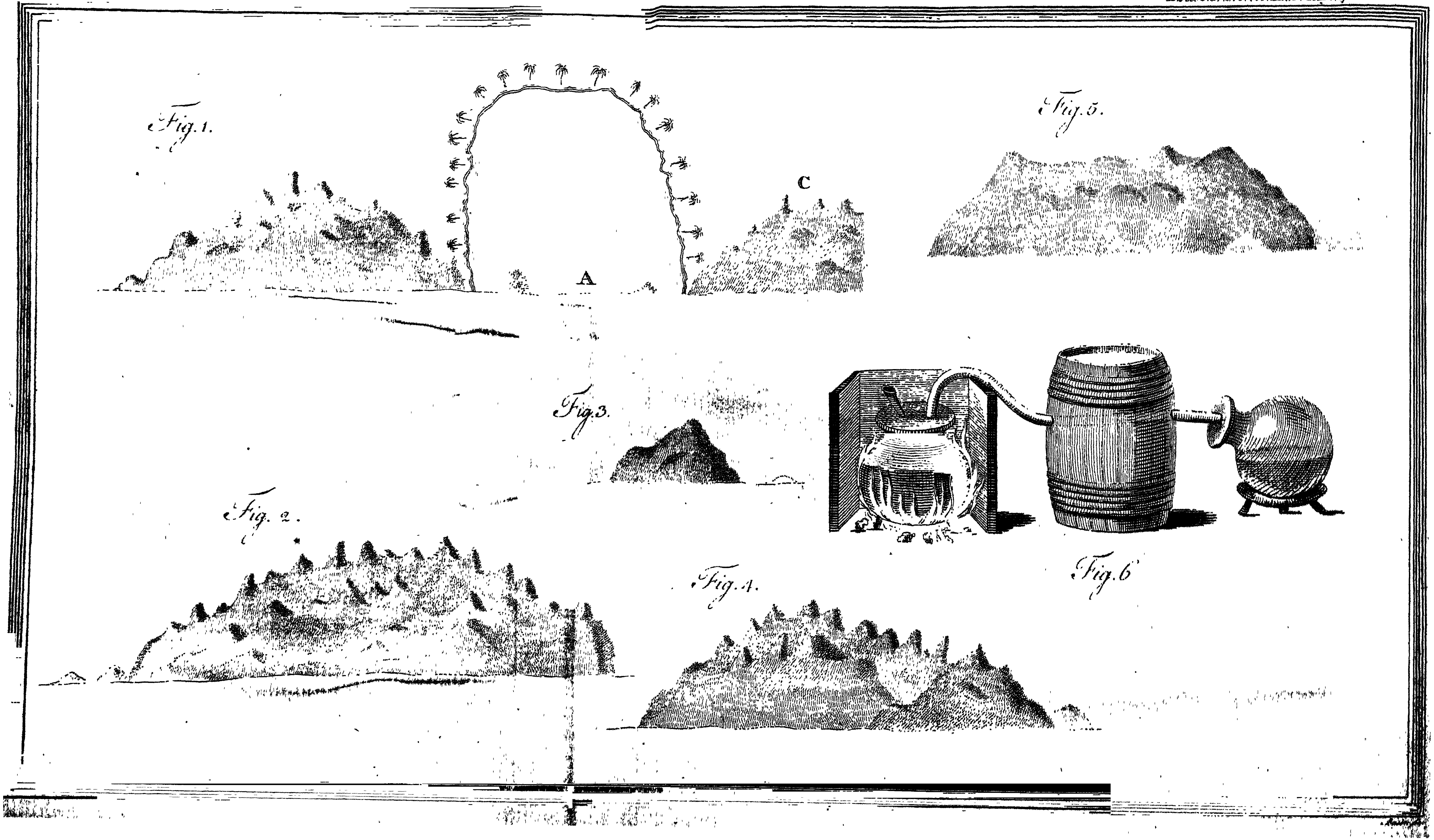


Fig. 1.
A
C
Fig. 5.
Fig. 3.
Fig. 2.
Fig. 6
Fig. 4.

cape is high land, and may be ſeen in fair weather about 12 leagues.

N. B. When Cape Aden bore N. N. W. ½ W. about 5 miles, I had 40 fathom, coarſe ſand and ſmall ſhells, the opening of the ſmall bay appeared like the narrow Streight of Babelmandel, N. W.

Thus ſheweth Cape Aden C, when the opening A bears W. N. W. ½ N. diſtance about 4 miles. Tab. V. Fig. 1.

Thus ſheweth Cape Aden, when coming from the eaſtward, at the diſtance of about 7 leagues, and when it bears about W. by N. Fig. 2.

To give any directions for ſailing through the narrow Streight of Babelmandel, and from thence to Mocha road, would be needleſs, as they are ſo extremely good in the Eaſt India Directory, as alſo for anchoring and ſailing into the road, with proper bearings, and diſtance to anchor from the town.

Thus ſheweth Cape Babelmandel, when it bears N. W. by W. diſtance 6 or 7 leagues, Fig. 3.

Thus ſheweth Cape Aden, when it bears W. by N. 10 or 11 leagues, Fig. 4.

From Mocha towards Judda, the iſlands of Jebbel-Zeker Aloric are pretty large, and may be ſeen in clear weather 7 or 8 leagues; they are ſix in number, the ſouthernmoſt lies in the latitude of 13° 45′ N. and bears from Mocha N. W. by W. nearly, diſtance

about 40 miles. A little to the northward of thoſe iſlands lies Jebbel-Zeker, a very high large iſland, that may be ſeen in fair weather 12 or 13 leagues. Very near this iſland, of the N. E. ſide, lie three ſmall ones, not diſcernible at a diſtance of 4 leagues. The N. end of the large iſland Jebbel-Zeker lies in the latitude of 14° 10′ N.

In coaſting along the Arabian ſhore, abreaſt of the large iſland, care muſt be taken not to come too near the ſhore, as there is a ſhoal water, between the moſque of Cape Name and Cape Namel, 7 or 8 miles from the ſhore, and foul ground, with overfalls. The edge of this bank is very ſteep too; for when I was from the ſhore about the above diſtance, I had preſently from 20 to 7 fathom water, and then 6 fathom. I immediately hauled off, and deepened my water again very ſoon, to 13, 14, and then 20 fathoms, as may be ſeen by the ſoundings in the draught, Tab. IV.

The true courſe from Jebbel-Zeker to the Subugars is N. W. by N.; diſtance 20 leagues. Thoſe iſlands are extremely well laid down in three different charts I have met with for the Red Sea; they trench away about N. N. W. and S. S. E. and extend from N. to S. about 20 miles; they are nine in number, and not very high, however, I believe they may be ſeen, in clear weather, from the maſt-head 7 or 8 leagues; the latitude of the S. and N. ends 14° 57′ to 15° 15′ N.

N. E. of thoſe iſlands lies a low white iſland (which I call Sandy Iſland), environed all round with ſhoal water; to the ſouthward of which, the ſhoal ſeemed, from the maſt-head, to extend from [illegible] 3 or 4 miles. I paſſed it at about 6 miles

diſtance,

diſtance, and never had leſs than 26 fathom, ſandy ground. Two or 3 miles within me, appeared like very ſhoal water. Its latitude is 15° 22′ N.

About 40 miles N. N. E. from the Subugars, lies the Iſland Comoran, a very low blackiſh iſland, trenching away N.W. and S.S.E. excepting the north end, which turns off ſuddenly, and ſtretches away N. N. E. When at the diſtance of about 5 miles from this iſland, I had 23 fathom water, hard ſand and gravel ſtones, at the ſame time it bore E.N.E. and, when the body bore N. E. diſtance about 6 miles, I ſaw a large ſquare white houſe near the waterſide.

N. W. by N. by the compaſs, from the Subugars, lies the Iſland Jebbel-Tar, diſtance about 25 miles. This iſland is of a moderate height, and may be ſeen 9 or 10 leagues from the maſt-head, in clear weather; its latitude is about 15° 36′ N. and when it bore W. about 10 miles, I had 33 fathom water, a ſandy bottom.

After taking your departure from this iſland, when bound to Judda, the beſt courſe to ſteer is N. N. W. ½ W. which is near the mid channel; by ſo doing, you may run boldly on all night, without fear.

From Jebbel-Tar to the ſmall iſlands on the Arabian ſide, laid down in about 18° N. latitude, I made the courſe N. 22° 49′, W. diſtance 159 miles. It was about ſun-ſet when I firſt ſaw two of thoſe iſlands; they then bore from N. E. to S. E. by S. diſtance from the neareſt of them about 6 miles, and breakers a little to the ſouthward about 5 miles. The ſouthernmoſt of theſe iſlands lies in the latitude of 18° 2′ N. according to my bearing and diſtance at

ſun-ſet; they are very low, but long, and ſtretch to the northward; I had no ſoundings, 80 fathom at the above diſtance.

From the above iſlands I ſaw no dangers, till I was in the latitude of 19° 24′ N. (upon the Ethiopian ſhore), when I ſaw breakers; and a little to the N. W. of them I ſaw a low ſandy iſland, that cannot poſſibly be ſeen at a greater diſtance than 6 or 7 miles; at the ſame time I ſaw two high iſlands to the weſt and northward of them, diſtance 8 or 10 leagues.

And, upon the eaſtern or Arabian ſhore, in the latitude of 20° 14′ N. I ſaw a low ſmall ſandy iſland; and 3 or 4 miles farther to the northward, another low ſandy iſland, about the ſize of the former, neither of which can be ſeen, in clear weather, at a greater diſtance than 3 leagues. At the ſame time, a little to the northward and weſtward withal, I ſaw breakers very plain from the ſhip's deck; thoſe iſlands were called (by the pilot I had on board) Marſaharam; he at the ſame time informed me, that it was very dangerous to go to the eaſtward of them, it being nothing but ſhoals and rocks. When you are to the northward of theſe iſlands and breakers, you will ſee the high land of Gooſs, at the diſtance of 14 leagues, the approaching of which is very dangerous without a pilot, as well as all the reſt of the coaſt quite to Judda, it being ſo incumbered with rocks and ſhoals; and what makes it the more hazardous is, there being no ſoundings till you come at once upon a hard ſteep ſand-bank, or a ledge of rocks; therefore, it will be abſolutely neceſſary to take a pilot on board, in or about 20° N. latitude, if poſſible; but ſhould you not be ſo

fortunate

fortunate as to get one before you come near Judda, it would be moſt certainly prudent to keep 30 or 40 miles from the ſhore, at leaſt ſo far that you can but juſt diſcern the high land of Goofs and Gedan, at which diſtance there is no danger. Although this may appear a great diſtance for the pilots to come off to the ſhip, yet they will immediately do it as ſoon as they hear your gun, and not till then.

It is indeed amazing, and almoſt incredible to be told, how far theſe pilots will hear the guns on a ſtill morning or evening, which are theproper times for the guns to be fired. Obſerve to fire the firſt as ſoon as you ſee the ſun appear in the horizon, and the ſecond as ſoon as the lower limb is juſt out of the water; in the evening, the firſt as ſoon as the lower limb touches the water, and the ſecond when the upper limb is below the horizon. Four firings in one day is all that are neceſſary; but they are to be repeated every day till you get a pilot. They know pretty near the time the India ſhips will arrive, and go down to the water ſide every night and morning, and juſt as the ſun is riſing or ſetting, they lay their ear cloſe to the ground for three or four minutes, and pretend to ſay, that if a ſhip is not more than two or two and a half degrees diſtance when the gun is fired, they can either hear the report or find the ground ſhake under them; upon which they take a boat and come off to pilot you in. This may ſeem a little extraordinary to a perſon that never was there; but, however ſtrange it may appear, I was aſſured by a gentleman of undoubted veracity, that he run by the log 95 miles from the time of firing his two guns in the morning, till he ſaw the pilot in the evening; and when he

came on board, he declared that he heard the two guns that morning at ſun-riſing, upon the ſtrength of which, he took his boat and put off.

To ſail into Judda harbour, or rather road, without a pilot, would be impoſſible for a ſtranger, there being ſo many ſand-banks and ſhelves of rocks; but when you are in, it is one of the ſafeſt places that can poſſibly be; you may make your ſhip faſt with any old junk, and there is no danger, though you are ſurrounded with nothing but rocks and ſands.

The beſt bearing for anchoring is the great Moſque E. by S. and the extremes of the land from S. by E. to N. N. W. diſtance from the landing-place about two miles.

	°	′	″	
Latitude of Judda	21	28		N.
Longitude Ditto	39	26	45	E.
Variation of the compaſs	11	52		W.

I left Judda the 30th of July 1769, and paſſed by the grap ſhoals (lying in the latitude of 21° 20′ N.) at about a mile's diſtance, from which I took my departure and made my courſe the firſt day S. 24° W. diſt. 56 miles; the ſecond day, S. 15° E. diſt. 100 miles. On the third day about 6 o'clock in the afternoon I ſaw a very high land on the Ethiopian ſhore (about the latitude of 18° 38′ N.) and ſome ſmall iſlands a little to the Northward of it; the high land bore about W. N. W. and the ſmall iſlands N. W. diſt. from the high land about 10 leagues; we had then run from the Grab ſhoal 195 miles.

From

From the ſhip's place at 6 P. M. run about 68 min. S. S. E. by the compaſs, and then made a low long ſandy iſland bearing S. W. about 8 miles on the ſame ſhore, on the South end of which are breakers that may be ſeen 8 or 9 miles; the middle of this iſland lies in or near the latitude of 16° 42′ N. from thence to Jebbel-Tar the diſtance is 128 miles, in a direct S. E. courſe by the compaſs.

Thus ſheweth the iſland of Jebbel-Tar, when it bears W. by S. diſt. about 10 miles; Tab. V. fig. 5.

From Jebbel-Tar to Jebbel-Zeker, the courſe is by the compaſs S. E. by S. diſtance about 100 miles: the paſſage to the Weſtward of it I had been informed was a very good one, but I find by experience it is not ſo good as that to the Eaſtward of it; nor do I think it ſo ſafe to go that paſſage in the night, except you are cloſe to the iſland before it is dark, and well to the Southward of the large Jebbel-Zeker, ſo that you can ſee the Southernmoſt of the ſmall Jebbel-Zeker Alories, as I found a very ſtrong current ſetting upon the Abyſſinian ſhore, and to the Southward withal. On the evening of the 4th of Auguſt at ſun-ſet, the large Jebbel-Zeker bore E. by S. diſtance about 4 leagues, and the Southernmoſt one S. S. E. half E. Having a very fine wind, and wanting to be at Mocha very much, I carried a moderate ſail, and ſteered from ſun-ſet till 8 o'clock South about 7 miles, from 8 till 10 S. by E. 7 miles, from 10 till 12 S.S.E. 7 miles, and then bore away, concluding myſelf well to the Southward of all the iſlands of Jebbel-Zeker Alorie; and indeed had my draught been good, and had I met with no current, I ſhould have been clear of every thing according to my

my run, which gave me about 5 miles to the Southward of the Southernmoſt Jebbel-Zeker; but to my great ſurprize, juſt as I was bearing away, I ſaw two iſlands right a-head, within about half a mile of us. We were then going at the rate of 4 knots: I immediately hauled off to the S. W. and ſoon after S. till I thought we were quite clear, and then (about two o'clock) going to bear away a ſecond time, we diſcovered two very ſmall iſlands, leſs than a mile from us, right a-head, upon which I ſhortened ſail, and laid the ſhip's head to the Weſtward till day-light, when I perceived myſelf ſurrounded with a parcel of ſmall iſlands (not laid down in any chart), about half way between Jebbel-Zeker Alories and the Abyſſinian ſhore. It falling little wind, and the current driving me very faſt upon the Abyſſinian ſhore, I let go my anchor in 27 fathom ſandy bottom, and there laid till 10 in the morning, when a breeze of wind ſprung up from the Northward; I then immediately hove up my anchor, and ſtood over for Mocha ſteering N. 77° E. diſtance 39 miles: when at anchor, I was about 3 miles from the Abyſſinian ſhore, and about half a mile from a large rock, or rather a ſmall iſland. While I was among theſe iſlands, I ſaw no breakers or ſhoal water; the leaſt water we had was 27 fathom, and never more than 40 fathom, and moſtly ſandy ground. Moſt of theſe ſmall iſlands I have marked in my chart, pretty near as they bear from each other; the diſtance from the Abyſſinian to the Arabian ſhore is not more than 40 or 45 miles (about the latitude of 13° 25′ N.) though in moſt charts it is made to meaſure from 70 to 80 miles, which certainly muſt be a great deal

too much; for, ſuppoſing an error in my run from ſhore to ſhore 10 miles, it would only make the diſtance 50 miles, which is 25 or 30 miles leſs than the charts give.

Latitude obſerved at Mocha	13° 23′ N.
Variation of the compaſs	12° 33′ W.

XII. *An easy Method to distill fresh Water from Salt Water at Sea; by Capt.* Newland.

Read March 12, 1772.

THE materials necessary for this process are the following; a copper or iron pot of 15 or 20 gallons, an empty cask, some sheet lead, a small jar, a few wood-ashes or soap, and billet-wood for fewel. See Tab. V. fig. 6. where A is the still or pot; B the pipe or worm; C the worm-tub; D the receiver; EE the fagong or fire-place; and *f* the plug-hole to put in water.

FIRST, In order to make my pipe or worm B, I took as much sheet lead as I thought was sufficient for the purpose, and beat it on a sponge staff to make it round: this being done, I was somewhat at a loss for solder; however, I supplied that deficiency with good paste and dungeree (or thin canvas) laid well on, and over that, a second coat of paste and dungeree, and then a covering of small cered line hove close together and very tight round, over which I put my third coat of paste and dungeree, which I found, to my great satisfaction, was sufficient to keep it from blowing. The next thing was to fix my pipe B in the pot or still head A. When I had well secured the pot in the fagong EE. I filled it about two thirds full of salt water (about 15 gallons), with which I mixt two or three double handfulls of wood-ashes, and stirred it well together, in order to soften the salt water; I then fixt the lid (which was made

made of plank 3 inches thick) in which there are two holes, one for the end of the pipe, the other to put in water as occaſion requires, without taking off the lid. It muſt be well obſerved, that the end of the pipe is not put more than 2 or 3 inches within the ſtill head; for, ſhould it be put too far in, when the water boils, the bubbles or ſaline particles get into the end of the pipe, and make the water brackiſh in the receiver. D. To prevent the ſteam from coming out at the plug-hole *f* or lid A, I made a kind of mortar, with wood-aſhes, ſalt water, and rope cut very ſmall and beat well together, and then applied it thereto, which anſwered my purpoſe extremely well. Now my pipe is fixt in the ſtill-head, I ſhall proceed in the next place to carry it through the worm tub C into the receiver D. My worm-tub is nothing more than an empty caſk with one of the heads taken out, and in each ſide a round hole cut, of about 3 inches diameter, for the pipe B to paſs through into the receiver D, which is fixt at a little diſtance from the tub C. The receiver has alſo a wooden lid like that of the ſtill-head, with a hole in it for the end of the worm to go through into the receiver D; care muſt be taken, that no ſteam comes out there, as well as at the ſtill-head. An empty jar will anſwer the purpoſe of a receiver very well. Notwithſtanding the pipe B paſſes through the tub C of cold water, your jar will be very hot; I therefore thought it neceſſary to keep a perſon continually wetting it with cold water, which not only kept the jar from breaking, but made the freſh water cold and fit for uſe immediately after the ſtill was

taken off. The foregoing directions ſtrictly obſerved, a quantity of 8 or 10 gallons will be produced every day, each day containing 12 hours.

Note. Every five or ſix hours you muſt repleniſh the ſtill with about five gallons of water, as I found my firſt ſtock conſumed about a gallon per hour by boiling.

XIII. *Observations on the milky Appearance of some Spots of Water in the Sea; by* the Same.

Read March 12, 1772.

IT has been remarked by several navigators, on their passage from Mocha to Bombay, Surat, &c. that they had discovered in the night spots of water as white as milk, and could never assign any reason for it; and many have been so much alarmed, that they have immediately hove to and sounded; but I never heard of any body ever getting ground. In my passage across those seas in the Kelsall, I discovered all of a sudden, about 8 o'clock in the evening, the water all round me as white as milk (intermixt with streaks or serpentine lines of black water). I immediately drew a bucket of it, and carried it to the light, where it appeared just as other water; I drew several more, and found it the same: some I kept till the next morning, when I could perceive no difference from that alongside. We had run by the log 50 min. from the time we first observed it till daylight, and during all that time the water continued white as milk, but at full daylight it was of its usual colour. The next evening about 7 o'clock the water appeared again as white as before; I then drew another bucket and carried it to a very dark place, and holding my head close to the bucket could perceive,

ceive, with my naked eye, an innumerable quantity of animalcules floating about alive, which enlightened that ſmall body of water to an amazing degree. From thence I conclude that the whole maſs of water muſt be filled with this ſmall fiſh ſpawn or animalcules, and that this is without all doubt the reaſon of the water's appearing ſo white in the night-time. We run by the log, from the time we firſt ſaw it till the latter part of the ſecond night (the time we loſt ſight of it) about 170 miles.

N. B. Latitude about 15° 10′ N. and S. W. diſt. from Cape Aden 12° 18′ E.

On the 30th of Auguſt 1769, at 3 o'clock in the morning, I ſaw a comet 8° 20′ from Aldebaran S. W. and the tail ſtreaming to the Weſtward.

I made the meridian diſtance from Cape Aden to Striking Sounding on the Malabar coaſt (in the lat. of 14° 2′ N.) 27° 31′ E.

XIV. *A Letter from Mr.* Peter Dollond, *to* Nevil Mafkelyne, *F. R. S. and Aftronomer Royal; defcribing fome Additions and Alterations made to* Hadley's *Quadrant, to render it more ferviceable at Sea.*

Reverend Sir,

Read March 29, 1772.

THE particular attention, you have always fhown to any improvement tending to the advantage of aftronomy or navigation, makes me take the liberty to trouble you with an account of fome additions and alterations I have lately made to the Hadley's quadrant.

The general ufe of this inftrument at fea is fo well known, that no mention need be made of the importance of any improvements in the conftruction, that may render the obfervation more exact, and occafion more frequent opportunities of making them.

The glaffes of the Hadley's quadrant fhould have their two furfaces perfect planes, and perfectly parallel to each other. From feveral years practice in grinding thefe glaffes, I have found out methods of making them to great exactnefs; but the advantage, that fhould arife from the goodnefs of the glaffes, has

has oftentimes been defeated by the index glaſs being bent by the braſs frame that contains it: to prevent this, I have contrived the frame, ſo that the glaſs lies on three points, and the part that preſſes againſt the front of the glaſs has alſo three points exactly oppoſite to the former. Theſe points are made to confine the glaſs by three ſcrews at the back, that act exactly oppoſite to the points between which the glaſs is placed. This little contrivance may be of ſome uſe; but the principal improvements are in the methods of adjuſting the glaſſes, particularly for the back obſervation.

The method hitherto practiſed for adjuſting that part of the inſtrument, by means of the oppoſite horizons at ſea, has been attended with ſo many difficulties that it has ſcarce ever been uſed; for ſo little dependance could be made on the obſervations taken this way, that the beſt Hadley's ſextants made for the purpoſes of obſerving the diſtances of the Moon from the Sun or fixed ſtars, have been always made without the horizon glaſs for the back obſervation; for want of which, many valuable obſervations of the Sun and Moon have been loſt, when their diſtance has exceeded 120 degrees.

To make the adjuſtment of the back obſervation eaſy and exact, I have applied an index to the back horizon glaſs, by which it may be moved into a parallel poſition to the index glaſs, in order to give it the two adjuſtments, in the ſame manner as the fore horizon glaſs is adjuſted. Then, by moving the index to which the back horizon glaſs is fixed, exactly 90 degrees (which is known by the diviſions made for that purpoſe) the glaſs will be thereby ſet

at right angles to the index glaſs, and conſequently will be properly adjuſted for uſe, and the obſervations may be made with the ſame accuracy by this, as by the fore obſervation.

To adjuſt the horizon glaſſes in the perpendicular poſition to the plane of the inſtrument, I have contrived to move each of them by a ſingle ſcrew, that goes through the frame of the quadrant, and is turned by means of a milled head at the back, which may be done by the obſerver while he is looking at the object.

To theſe improvements, Sir, I have added your method of placing darkening glaſſes behind the horizon glaſſes, which you have been ſo kind as to give me liberty to apply to my inſtruments. Theſe glaſſes, which ſerve for darkening the object ſeen by direct viſion, in adjuſting the inſtrument by the Sun or Moon, I have placed in ſuch a manner as to be turned behind the fore horizon glaſs, or behind the back horizon glaſs, that they may be uſed with either; there are three of theſe glaſſes of different degrees of darkneſs; the lighteſt or paleſt I do imagine will be of uſe in taking the Sun's altitude when the horizon appears glaring, which I believe often happens by the reflection of the ſea.

If theſe additions and alterations ſhould be thought to be real improvements, which I cannot doubt, Sir, if they are honoured with your approbation, I hope they may ſerve in conjunction with thoſe improvements you have made yourſelf in reſpect to the obviating any poſſible errors in the paralleliſm of the planes of the index glaſs, and in regard to the adjuſtment of the teleſcope parallel to the plane of the

 quadrant,

quadrant, to extend the uſe of this moſt valuable nautical inſtrument, and to add to the exactneſs of the celeſtial obſervations taken with it to determine the longitude at ſea. But of theſe particulars I need ſay no more, ſince you are, without doubt, in every reſpect, the propereſt perſon to give an account of them.

I am, SIR,

Your moſt obedient,

humble ſervant,

London, February 25, 1772.

Peter Dollond.

XV. Re-

Received May 22, 1772.

XV. *Remarks on the* Hadley's *Quadrant, tending principally to remove the Difficulties which have hitherto attended the Use of the Back-observation, and to obviate the Errors that might arise from a Want of Parallelism in the two Surfaces of the Index-Glass. By* Nevil Maskelyne, *F. R. S. Astronomer Royal.*

Read May 28, 1772.

THE back observation with Hadley's quadrant being founded on the same principles, and in theory, equally perfect with the fore-observation, and being at the same time necessary to extend the use of the instrument up to 180 degrees (it being impracticable to measure angles with any convenience beyond 120 degrees with the fore-observation) it may seem surprizing that it hath not been brought equally into general use, more especially since the method of finding the longitude by observations of the Moon, has been practised at sea for some years past; since this method would receive considerable advantage from the use of the back-observation in taking distances of the Sun and Moon between the first and last quarter, could such

observations be as much depended upon as the fore-observations. The causes of this seem to have been principally these two, the difficulty of adjusting the back horizon glass, and the want of a method of directing the sight parallel to the plane of the quadrant. The back horizon glass, like the fore-one, requires two adjustments: the first, or common one, disposes it at right angles to the index glass, when the index stands at (o) upon the arch; which is usually performed by setting (o) of the index of the arch of the quadrant by double the dip of the horizon of the sea, and then holding the quadrant vertical with the arch downwards, and turning the back-horizon glass about, by means of its lever or perpetual screw, till the reflected back horizon appears to coincide with the fore-horizon seen directly. But this operation is so difficult in practice with the back-horizon glass wholly silvered, except a small transparent slit in the middle, as it has been usually made, that few (if any) persons have ever received proper satisfaction from it. If the back-horizon-glass was silvered in every respect like the fore-horizon-glass (which it ought to be) the upper part being left unsilvered, and a telescope was applied to it, perhaps this adjustment might be rendered somewhat easier and more exact; but it could not even thus be made so exact as the adjustment of the fore-horizon-glass may, by making use of the Sun's limbs.

The second adjustment of the back-horizon-glass, in the common construction of the quadrant, is still more troublesome, since it cannot be executed without setting the index 90 degrees off the arch, in order to place the index-glass parallel

allel to the back-horizon-glaſs; when this adjuſtment may be performed in the ſame manner as the correſponding adjuſtment of the fore-horizon-glaſs. But the bending of the index, that follows the ſetting it off the arch, is a very diſagreeable circumſtance, having a tendency, eſpecially on board of ſhip, to expoſe both the index and centre work to damage; and may even, without extraordinary precautions taken by the inſtrument maker in placing the plane of the index-glaſs exactly according to the length of the index, diſturb its perpendicularity to the plane of the quadrant: on theſe accounts it would be much better if this adjuſtment of the back-horizon-glaſs could be performed, like thoſe of the fore-horizon-glaſs, with the index remaining upon the arch of the quadrant. Fortunately, this *deſideratum* has been lately effected by an ingenious contrivance invented by Mr. Dollond, which he has given an account of in a letter addreſſed to me*, which I have preſented to this Society, by means of an additional index applied to the back-horizon-glaſs; whereby both the adjuſtments may be made by the ſame obſervations and with nearly the ſame exactneſs as thoſe of the fore-horizon-glaſs: for a farther knowledge of which, ſee the account itſelf.

Beſides the difficulty of adjuſting the back-horizon-glaſs, the want of a method of directing the line of ſight parallel to the plane of the quadrant has proved alſo a conſiderable obſtacle to the uſe of the back-obſervation: this will eaſily appear from the following propoſition, that the error of the angle meaſured ariſing from any ſmall de-

* See the XIV th paper, which immediately precedes this.

viation,

viation of the viſual ray from a paralleliſm to the plane of the quadrant, is to twice an arch equal to the verſe-ſine of the deviation; as the tangent of half the angle meaſured by the quadrant is to radius, very nearly. Thus a deviation of 1° in the line of ſight, will produce an error of about 1′ in meaſuring an angle of 90°, whether by the fore or back obſervation; but the ſame deviation will produce an error of 4′ in meaſuring an angle of 150°, of 6′ in taking an angle of 160°, and 12′ in taking an angle of 170°. Hence a pretty exact adjuſtment of the line of ſight, or axis of the teleſcope, is requiſite in meaſuring large angles, ſuch as thoſe are taken by the back obſervation: and therefore a director of the ſight ought by no means to be omitted in the conſtruction of the inſtrument (as it commonly has been ſince Mr. Hadley's time, though recommended by him), except a teleſcope be made uſe of, which if rightly placed anſwers the ſame purpoſe better, eſpecially in obſerving the diſtance of the Moon from the Sun between the firſt and laſt quarter. The director of the ſight may be placed exact enough by conſtruction; but the teleſcope cannot, and Mr. Hadley, not having been aware of the importance of an exact poſition of it, has accordingly given no directions for the placing it. I ſhall therefore endeavour to ſupply this defect in the following remarks.

In the firſt place, I would by all means recommend an adjuſting piece to be applied to the teleſcope, whereby its axis may be brought parallel to the plane of the quadrant: in the next place, the back-horizon-glaſs ought to be ſilvered in the ſame manner as the fore-horizon-glaſs: and thirdly, two thick ſilver wires ſhould be placed within the eye-tube

in the focus of the eye-glaſs parallel to one another, and to the plane of the quadrant. If they were put at ſuch a diſtance as to divide the diameter of the field of view into three equal parts, it might be as convenient as any other interval. In this manner wires were placed in the teleſcope by Mr. Hadley, as appears by his account of the inſtrument in Philoſ. Tranſ. N° 420. Theſe wires are to be adjuſted parallel to the plane of the quadrant, by turning the eye-tube round about which contains the wires, till they appear parallel to the plane of the quadrant. The axis of the teleſcope, by which is meant the line joining the centre of the object-glaſs and the middle point between the two wires, is to be adjuſted parallel to the plane of the quadrant by either of the two following methods.

1ſt method. When the diſtance of the Moon from the Sun is greater than 90 degrees, by giving a ſweep with the quadrant and moving the index, bring the neareſt limbs to touch one another at the wire neareſt the plane of the quadrant. Then, the index remaining unmoved, make the like obſervation at the wire fartheſt from the plane of the quadrant; and note whether the neareſt limbs are in contact as they were at the other wire: if they are, the axis of the teleſcope is parallel to the plane of the quadrant: but if they are not, it is inclined to the ſame, and muſt be corrected as follows. If the neareſt limbs of the Sun and Moon ſeem to lap over one another at the wire fartheſt from the plane of the quadrant, the object end of the teleſcope is inclined from the plane of the quadrant, and muſt be altered by the adjuſtment made for that purpoſe: but, if the neareſt limbs

limbs of the Sun and Moon do not come to touch one another at the wire fartheſt from the plane of the quadrant, the object end of the teleſcope is inclined towards the plane of the quadrant, and muſt be altered by the adjuſtment accordingly. Let theſe operations be repeated until the obſervation is the ſame at both the parallel wires, and the axis of the teleſcope will be adjuſted parallel to the plane of the quadrant. In like manner, the axis of the teleſcope may be alſo adjuſted parallel to the plane of the quadrant for the fore-obſervation.

Second method. Set the index to (o) and hold the plane of the quadrant parallel to the horizon of the ſea, with the divided arch upwards, the two wires being parallel to, and including both the direct fore-horizon, and the reflected back-horizon, between them. Raiſe or lower the plane of the quadrant until the direct and reflected horizons coincide together: if the coincidence happens in the middle between the two wires, or rather, to be more exact, above the middle by ſuch a part of the field of view as anſwers to the number of minutes in the depreſſion of the horizon (which may be eaſily eſtimated if the angular interval of the wires be firſt found by experiment, in manner hereafter mentioned) the axis of the teleſcope is parallel to the plane of the quadrant; but if it does not, the line of ſight is inclined to the plane of the quadrant, and muſt be corrected as follows. If the direct and reflected horizons, when they coincide, appear higher above the middle between the wires, than what the quantity of the depreſſion of the horizon amounts to, the object end of the teleſcope is inclined from the plane of the quadrant, and muſt be altered by the adjuſtment made for that purpoſe;

but

but if the two horizons appear to coincide in a lower part of the field of the telescope, the object end of the telescope is inclined towards the plane of the quadrant, and must be altered by the adjustment accordingly. Repeat these operations till the two horizons appear to coincide above the middle between the two wires, by the quantity of the depression of the horizon, and the axis of the telescope will be adjusted parallel to the plane of the quadrant. In order to find the angular interval between the wires, hold the quadrant perpendicular to the horizon, as in observing altitudes; and turn about the eye-tube with the wires until they are parallel to, and include, the direct fore-horizon and reflected back-horizon between them. Move the index from (o) along the divided arch, at the same time raising or lowering the telescope by the motion of the quadrant until the direct horizon appears to coincide with the upper wire, and the reflected back-horizon with the lower wire; the number of degrees and minutes shewn upon the arch, increased by double the depression of the horizon, will be the angular interval of the wires; its proportion to the depression of the horizon will be therefore known; and hence the space in the field of the telescope answering to the depression of the horizon, may be easily estimated near enough for adjusting the axis of the telescope in the manner before-mentioned. The first of the two methods here given for adjusting the position of the telescope will probably be found most convenient; and the greater the distance of the Sun and Moon is, the more nearly may the adjustment be made, because the same deviation of the axis of the telescope will cause a greater error.

 The

The telescope should be fixed by the instrument-maker so as to command a full field of view when the instrument is placed at 90° if the instrument be an octant, or 120° if it be a sextant; because the index-glass then stands more oblique with respect to the incident and reflected rays, and consequently the field of view of the telescope, as far as it depends upon the index-glass, will be more contracted than in any other position of the index: but if there is a fair field of view in this case, there necessarily must be so in every other position of the index.

The two parallel wires will be very useful on many occasions, as well in the fore as the back observation. In taking the altitude of the Sun, Moon, or star, direct the sight towards the part of the horizon underneath, or opposite to the object, according as you intend to observe by the fore or back observation, and hold the quadrant that the wires may constantly appear perpendicular to the horizon, and move the index till you see the object come down towards the horizon in the fore-observation, or up to it in the back-observation, and turn the instrument in order to bring the object between the wires; then move the index till the Sun or Moon's limb, or the star touch the horizon. The nearer the object is brought to an imaginary line in the middle between the wires (it is indifferent what part of the line it is brought to) and the truer the wires are kept perpendicular to the horizon, the more exact will the observation be. In the fore-observation, the object appears in its real position; but in the back-observation, the object being brought through the zenith to the horizon, the real upper-limb will appear the lowest;

and

and the contrary. Either limb of the Sun may be ufed in either obfervation; but it will be moft convenient in general to make the Sun appear againft the fky, and not againft the fea; and then the objects appearing inverted through the telefcope, the Sun will appear loweft, and the horizon higheft. The obferved altitude is to be corrected for dip, refraction, and Sun's femi-diameter, as ufual.

In taking the diftance of the neareft limbs of the Sun and Moon, whether by the fore or back-obfervation, having firft fet the index to the diftance nearly, by the help of the Nautical Almanac, and brought the Moon to appear anywhere on or near the diameter of the field of view of the telefcope, which bifects the interval between the wires, give a fweep with the quadrant, and the Sun and Moon will pafs by one another; if in this motion the neareft limbs, at their neareft approach, juft come to touch one another, without lapping over, on or near any part of the diameter of the field of the telefcope which bifects the interval between the wires, the index is rightly fet; but if the neareft limbs either do not come to meet, or lap over one another, alter the index, and repeat the obfervation till the neareft limbs come to touch one another properly. This method of obferving will be found much more eafy and expeditious than without the wires, fince in that cafe it would be neceffary to make the limbs touch very near the centre of the telefcope, but here it is only neceffary to make them do fo anywhere on or near the diameter of the field of the telefcope which bifects the interval between the two wires.

 The

The ſame method may be uſed in taking the Moon's diſtance from a fixed ſtar.

It may not be amiſs here to make ſome remarks on the rules that have been uſually given for obſerving the Sun's altitude, both with the fore and back-obſervation, which have all been defective, and to point out the proper directions to be followed, when a teleſcope is not uſed with two parallel wires to direct the quadrant perpendicular to the horizon, and to ſhew the principles on which theſe directions are founded.

Obſervers are commonly told, that in making the fore obſervation they ſhould move the index to bring the Sun down to the part of the horizon directly beneath him, and turn the quadrant about upon the axis of viſion; and when the Sun touches the horizon at the loweſt part of the arch deſcribed by him the quadrant will ſhew the altitude above the viſible horizon. I allow that this rule would be true, if a perſon could by ſight certainly know the part of the horizon exactly beneath the Sun; but, as this is impoſſible, the precept is incomplete. Moreover, in taking the Sun's altitude in or near the zenith, this rule entirely fails, and the beſt obſervers adviſe to hold the quadrant vertical, and turn one's ſelf about upon the heel, ſtopping when the Sun glides along the horizon without cutting it: and it is certain that this is a good rule in this caſe, and capable with care of anſwering the intended purpoſe. We have thus two rules for the ſame thing, which is a proof that neither of them is an univerſal one, or ſufficient in all caſes alone.

In

In taking the back-obſervation, obſervers have been adviſed either to turn the quadrant about upon the axis of viſion, or, holding the quadrant upright, to turn themſelves about upon the heel, indifferently. The true ſtate of the caſe is this; that, in taking the Sun's altitude, whether by the fore or back-obſervation, theſe two methods muſt be combined together; that is to ſay, the obſerver muſt turn the quadrant about upon the axis of viſion, and at the ſame time turn himſelf about upon his heel, ſo as to keep the Sun always in that part of the horizon-glaſs which is at the ſame diſtance as the eye from the plane of the quadrant: for, unleſs the caution of obſerving the objects in the proper part of the horizon-glaſs be attended to, it is evident the angles meaſured cannot be true ones. In this way the reflected Sun will deſcribe an arch of a parallel circle round the true Sun, whoſe convex ſide will be downwards in the fore-obſervation, and upwards in the back-obſervation, and conſequently, when, by moving the index, the loweſt point of the arch in the fore-obſervation, or the uppermoſt point of the arch in the back-obſervation, is made to touch the horizon, the quadrant will ſtand in a vertical plane, and the altitude above the viſible horizon will be properly obſerved.

The reaſon of theſe operations may be thus explained: the image of the Sun being always kept in the axis of viſion, the index will always ſhew on the quadrant the diſtance between the Sun and any object ſeen directly which its image appears to touch; therefore, as long as the index remains unmoved, the image of the Sun will deſcribe an arch everywhere equidiſtant from the Sun in the heavens, and conſe-

quently

quently a parallel circle about the Sun, as a pole; ſuch a tranſlation of the Sun's image can only be produced by the quadrant being turned about upon a line drawn from the eye to the Sun, as an axis; a motion of rotation upon this line may be reſolved into two, one upon the axis of viſion, and the other upon a line on the quadrant perpendicular to the axis of viſion; and conſequently a proper combination of theſe two motions will keep the image of the Sun conſtantly in the axis of viſion, and cauſe both jointly to run over a parallel circle about the Sun in the heavens; but when the quadrant is vertical a line thereon perpendicular to the axis of viſion becomes a vertical axis; and, as a ſmall motion of the quadrant is all that is wanted, it will never differ much in practice from a vertical axis; therefore the obſerver, by properly combining and proportioning two motions, one of the quadrant upon the axis of viſion, and the other of himſelf upon his heel, keeping himſelf upright (which gives the quadrant a motion upon a vertical axis) will cauſe the image of the Sun to deſcribe a ſmall arch of a parallel circle about the Sun in the heavens, without departing conſiderably from the axis of viſion.

If it ſhould be aſked, why the obſerver ſhould be directed to perform two motions rather than the ſingle one equivalent to them on a line drawn from the eye to the Sun as an axis? I anſwer, that we are not capable, while looking towards the horizon, of judging how to turn the quadrant about upon the elevated line going to the Sun as an axis, by any other means than by combining the two motions above-mentioned, ſo as to keep the Sun's image al-

ways in the proper part of the horizon-glaſs. When the Sun is near the horizon, the line going from the eye to the Sun will not be far removed from the axis of viſion; and conſequently the principal motion of the quadrant will be performed on the axis of viſion, and the part of the motion made on the vertical axis will be but ſmall. On the contrary, when the Sun is near the zenith, the line going to the Sun is not far removed from a vertical line, and conſequently the principal motion of the quadrant will be performed on a vertical axis, by the obſerver's turning himſelf about, and the part of the motion made on the axis of viſion will be but ſmall. In intermediate altitudes of the Sun, the motions of the quadrant on the axis of viſion and on a vertical axis will be more equally divided. Hence appears the reaſon of the method uſed by the beſt obſervers in taking the Sun's altitude when near the zenith by holding the quadrant vertical and turning about upon the heel, and the defects of the rules that have been commonly given for obſerving altitudes in other caſes.

As it may conduce to the ſetting this matter in a ſtill clearer light, I ſhall here deſcribe in order the ſeveral motions that will be given to the reflected image, by turning the quadrant about upon the axis of viſion, a vertical axis, or the line drawn from the eye to the Sun, ſucceſſively.

I. If the quadrant is turned about upon the axis of viſion, the ſame being directed to the point of the horizon exactly beneath or oppoſite the Sun, the image of the Sun will move from right to left, or from left to right, acroſs the

the horizon-glaſs, the ſame way as the arch of the quadrant is carried, both in the fore and back-obſervations, with a velocity which is to the angular velocity of the quadrant as the ſine of the Sun's altitude to the radius, deſcribing an arch convex downwards in both caſes; and when the motion of the Sun in this arch is parallel to the horizon, the quadrant is held truely perpendicular to the horizon, and conſequently in a proper poſition for taking the Sun's altitude. But, if the axis of viſion be directed to, and turned round a point in the horizon beſide the vertical circle paſſing through the Sun, the Sun's image, when its motion is parallel to the horizon, will be neither in the axis of viſion nor the Sun's vertical, but between both; at the ſame time, the plane of the quadrant will not be vertical, and the altitude found by bringing the Sun's image to touch the horizon will not be the true altitude.

II. If the quadrant be held perpendicular to the horizon, and turned about upon a vertical axis, or one nearly ſo, the Sun will deſcribe an arch convex downwards in the fore-obſervation, and upwards in the back-obſervation, the motion of the Sun being the ſame way as the axis of viſion is carried in both caſes, and being to the angular motion of the quadrant, as the verſe-ſine of the Sun's altitude to the radius in the fore-obſervation, but as the verſe-ſine of the ſupplement of the Sun's altitude to 180° to the radius in the back-obſervation. The Sun therefore will move ſlower than the axis of viſion is the fore-obſervation, and conſequently will be left behind, with

with respect to the axis of vision, or seem to move backwards; and the Sun will move quicker than the axis of vision in the back-observation, or will seem to get before it. When the motion of the Sun in this arch is parallel to the horizon, the plane of the quadrant coincides with the vertical circle passing through the Sun, and consequently the quadrant is in a proper position for taking the Sun's altitude. But if the quadrant be held a little deviating from the perpendicular position to the horizon, and turned about upon an axis, either vertical or only nearly so, the arch described by the Sun apparently will cut the horizon, but will never move parallel to it, and consequently the quadrant will not be brought into a proper position for observing the Sun's altitude.

III. If the quadrant be turned on the line going to the Sun as an axis, the reflected Sun will be kept constantly in the axis of vision, and will describe an arch of a parallel circle about the real Sun, with a velocity which is to the angular motion of the quadrant, as the sine of the Sun's altitude is to the radius; and when the motion of the reflected Sun is parallel to the horizon, the quadrant is vertical.

Hence naturally arise the three methods of taking an altitude, which have been mentioned before. In the first, the axis of vision is supposed always directed to one and the same part of the horizon, namely that which is in the Sun's vertical. In the second, the observer is required to hold the quadrant truly vertical, and to turn himself upon a vertical axis;

 but

but it is evident neither of these motions can be accurately performed. In the third method, the observer is only required to move both himself and the quadrant, so as to keep the Sun always in or near the axis of vision, which may be performed very well, because the axis of vision is a visible and certain direction for it. One exception, however, should be made to this general rule, namely, in taking the Sun's altitude when very low, by the back observation; in which case it will be best to use the second method, or else to hold the quadrant perpendicular by judgment; which will be much facilitated by using a telescope containing wires in its focus parallel to the plane of the quadrant, as described in p. 106: for, in this case, the perpendicular position of the quadrant cannot be attained so near by the method of turning the quadrant on a line going to the Sun as an axis, as it can by the other method.

It remains to treat of the errors which may arise from a defect of parallelism in the two surfaces of the index-glass, and to point out the means of obviating them in the celestial observations. It is well known, that if a pencil of parallel rays falls upon a glass whose two surfaces are inclined to one another, and some of the rays are reflected at the fore-surface, and others passing into the glass and suffering a reflection at the back-surface and two refractions at the fore-surface emerge again from the glass, these latter rays will not be parallel to those reflected at the fore-surface, as they would have been if the surfaces of the glass had been parallel, but will be inclined to the same. I find that the angle of their mutual inclination, which may be called the deviation of the

rays

rays reflected from the back-ſurface, will be to double the inclination of the ſurfaces of the glaſs (which is here ſuppoſed to be but ſmall), as the tangent of the angle of incidence out of air into glaſs, is to the tangent of the angle of refraction. Hence, in rays falling near the perpendicular, the deviation will be about three times the inclination of the ſurfaces; and if the angles of incidence be 50°, 60°, 70°, 80° or 85°, the deviations of the reflected rays will be about 4, 5, 7, 13 or 26 times the inclination of the ſurfaces, reſpectively. Had the deviation been the ſame at all incidences of the rays on the index-glaſs, no error would have been produced in the obſervation; becauſe the courſe of the ray would have been equally affected in the adjuſtment of the inſtrument, as in the obſervation. But, from what has been juſt laid down, this is far from being the caſe, the deviation increaſing according to the obliquity with which the rays fall upon the index-glaſs; ſo that in very oblique incidences of the rays, ſuch as happen in meaſuring a large angle by the fore-obſervation or a ſmall angle by the back-obſervation, the leaſt defect in the paralleliſm of the planes of the two ſurfaces of the index-glaſs may produce a ſenſible error in the obſervation.

What is here ſaid only takes place in the fulleſt extent, if the thickeſt or thinneſt edge of the index-glaſs, or, to expreſs the ſame thing in other words, the common ſection of the planes of the ſurfaces of the index-glaſs ſtands perpendicular to the plane of the quadrant; but, if the common ſection of the planes is inclined to the plane of the quadrant, the error ariſing from the defect of the paralleliſm of the

 ſurfaces

ſurfaces will be leſſened in the proportion of the ſine of the inclination to the radius; ſo that at laſt, when the common ſection becomes parallel to the plane of the quadrant, the error intirely vaniſhes. For this reaſon, Mr. Hadley very properly directed the thickeſt and thinneſt edges of the index-glaſs to be placed parallel to the plane of the quadrant. But, as it may well be queſtioned whether this care is always taken by the inſtrument-maker, and it cannot be ſuppoſed that the glaſſes can be ground perfect parallel planes, it would certainly be an advantage acquired to the inſtrument, could the error ariſing from a want of paralleliſm of the planes be removed in whatever poſition the common ſection of the planes ſhould be placed with reſpect to the plane of the quadrant. This will be effected for celeſtial obſervations, if the upper part of the index-glaſs be left unſilvered on the back, and made rough and blacked, the lower part of the glaſs being ſilvered as uſual, which muſt be covered whenever any celeſtial obſervations are made. Then, if the teleſcope be ſufficiently raiſed above the plane of the quadrant, it is evident that the obſervations will be made by the rays reflected from the fore-ſurface of the upper part of the index-glaſs, and conſequently, if the quadrant be adjuſted by making uſe of the ſame part of the index-glaſs, the obſervations will be true whether the two ſurfaces of the index-glaſs are parallel planes or not. The Sun or Moon may be thus obſerved by reflection from the unſilvered parts of the index-glaſs and horizon-glaſs, ſo that a paler darkening glaſs will ſuffice, and they will appear much diſtincter than from an index-glaſs wholly ſilvered with a deeper darkening

ing glaſs; for although the ſurfaces of a glaſs may be parallel, yet there always ariſes ſome little confuſion from the double reflection. Neither will the Moon appear too weak by two unſilvered reflections, even when her creſcent is very ſmall, except ſhe ſhould be hazy or clouded; and then the light may be increaſed by lowering the teleſcope ſo as to take in part of the ſilvered reflection of the index-glaſs, which in this caſe muſt be uncovered: the ſame is alſo to be underſtood with reſpect to the Sun, ſhould his light be too much weakened by hazineſs or thin clouds. The horizon-glaſſes ſhould be adjuſted, or the error of adjuſtment found by the Sun or Moon; the firſt will be in general the beſt object for the purpoſe; and, as the Sun or Moon ſeen directly through the unſilvered part of the horizon-glaſs will be much brighter than the image of the ſame ſeen by two unſilvered reflections, it muſt be weakened by a darkening glaſs placed beyond the horizon-glaſs, the reflected image being farther weakened, if neceſſary, by a paler darkening glaſs placed in the uſual manner between the index-glaſs and the horizon-glaſs.

If a quadrant was deſigned principally for taking the diſtance of the Moon from the Sun and fixed ſtars, and was not wanted for obſerving terreſtrial angles, it would be the beſt way to have none of the glaſſes ſilvered, but to leave the horizon glaſſes intirely tranſparent, and to put a red glaſs for an index-glaſs of the ſame matter with the darkening glaſſes, which would reflect light from the fore-ſurface only.

The Sun's altitude might alſo be obſerved with this inſtrument, either by the fore or back-obſervation; and

and the altitude of the Moon might be taken with it in the night. But the altitudes of ſtars could not be obſerved with it, nor the Moon's altitude in the day time, which would however be no great inconvenience, as theſe obſervations might be well enough ſupplied by common quadrants.

The following rules for the ſize of the glaſſes and the ſilvering them, and the height of the teleſcope may be of uſe. The index glaſs and two horizon-glaſſes ſhould be all of equal height, and even with one another in height both at top and bottom. The teleſcope ſhould be moveable parallel to itſelf nearer to or farther from the plane of the quadrant, and the range of its motion ſhould be ſuch that its axis when at the loweſt ſtation ſhould point about $\frac{1}{10}$th of an inch lower than the top of the ſilvering of the horizon-glaſſes, and when at the higheſt ſtation ſhould point to the height of the middle of the unſilvered part of the index-glaſs. The height of the glaſſes, and the quantity of parts ſilvered and parts unſilvered, ſhould vary according to the aperture of the object-glaſs, as in the following table; where the firſt column of figures ſhews the dimenſions in parts of an inch anſwering to an aperture of the object-glaſs of $\frac{3}{10}$ths of an inch in diameter; the ſecond column what anſwer to an aperture of the object-glaſs of $\frac{4}{10}$ths of an inch in diameter; and the third, what are ſuitable to an aperture of the object-glaſs of $\frac{5}{10}$ths of an inch in diameter.

Diameter

	Parts of an Inch		
Diameter of aperture of object-glaſs	,30	0,40	0,50
Height of glaſſes	,90	1,13	1,37
Height of ſilvered part of index-glaſs	,50	0,63	0,77
Height of unſilvered part of ditto	·40	0,50	0,60
Height of ſilvered part of horizon-glaſſes	25	0,33	0,42
Height of unſilvered part of ditto	,65	0,80	0,95

If the teleſcope has a common object-glaſs, the firſt aperture of $\frac{3}{10}$ths of an inch will be moſt convenient; but if it has an achromatic object-glaſs, one of the other apertures of $\frac{4}{10}$ths or $\frac{5}{10}$ths of an inch, will be moſt proper. The field of view of the teleſcope ſhould be 5 or 6 degrees, and the objects ſhould be rendered as diſtinct as poſſible throughout the whole field, by applying two eye-glaſſes to the teleſcope. The breadth of the glaſſes ſhould be determined as uſual, according to the obliquity with which the rays fall on them and the aperture of the object-glaſs.

I ſhall conclude this paper with ſome eaſy rules for finding the apparent angular diſtance between any two near land objects by the Hadley's quadrant.

To find the angular diſtance between two near objects by the fore-obſervation. Adjuſt the fore-horizon-glaſs by the object intended to be taken as the direct object; and the angle meaſured by the fore-obſervation on the arch of the quadrant between this object and any other object ſeen by reflection will be the true angle between them as ſeen from the centre of the index-glaſs. But, if the quadrant be already

already well adjuſted by a diſtant object, and you do not chuſe to alter it by adjuſting it by a near one, move the index, and bring the image of the near direct object to coincide with the ſame ſeen directly, and the number of minutes by which (o) of the index ſtands to the right hand of (o) of the quadrant upon the arch of the exceſs is the correction, which added to the angle meaſured by the arch of the quadrant between this direct object and any other object ſeen by reflection will give the true angular diſtance between them reduced to the centre of the index-glaſs.

To find the angular diſtance between two near objects by the back-obſervation.

It is ſuppoſed that the-horizon-glaſs is truly adjuſted; if it is not, let it be ſo. Obſerve the diſtance of the objects by the back-obſervation, and take the ſupplement of the degrees and minutes ſtanding upon the arch to 180 degrees, which call the inſtrumental angular diſtance of the objects; this is to be corrected as follows. Keep the centre of the quadrant or index-glaſs in the ſame place as it had in the foregoing obſervation, and obſerve the diſtance between the near object, which has been juſt taken as the direct object, and ſome diſtant object, twice; by making both objects to be the direct and reflected ones alternately, holding the divided arch upwards in one caſe and downwards in the other, ſtill preſerving the place of the centre of the quadrant. The difference of theſe two obſervations will be the correction, which added to the inſtrumental angular diſtance,

diſtance, found as above in the firſt obſervation between the firſt object and any other object ſeen by reflection, will give the true angular diſtance between them reduced to the centre of the index-glaſs.

But if you ſhould happen to be in a place where you cannot command a convenient diſtant object, the following method may be uſed.

The back-horizon-glaſs being adjuſted, find the inſtrumental angular diſtance between the objects; this is to be corrected by means of the following operations. Set up a mark at any convenient diſtance oppoſite or nearly ſo to the object which has been taken as the direct object; and looking at the direct object move the index of the quadrant, and bring the image of the mark to coincide with the direct object, and read off the degrees and minutes ſtanding on the arch of the quadrant, which ſubtract from 180 degrees, if (o) of the index falls upon the quadrantal arch; but add to 180 degrees, if it falls upon the arch of exceſs; and you will have the inſtrumental angular diſtance of the object and mark. Invert the plane of the quadrant, taking care at the ſame time not to change the place of its centre, and looking at the ſame direct object as before, move the index of the quadrant, and bring the image of the mark to coincide again with the direct object, and read off the degrees and minutes ſtanding on the arch, and thence alſo find the inſtrumental angular diſtance of the object and mark. Take the ſum of this and the former inſtrumental angular diſtance; half of its difference from 360° will be the correction, which added to the inſtrumental angular diſtance firſt found between the ſame direct object and the other object ſeen

 by

by reflection will give the true angular diſtance between them reduced to the centre of the index-glaſs.

It is to be obſerved, that if the mark be ſet up at the ſame diſtance from the quadrant as the direct object is, there will be no occaſion to invert the plane of the quadrant, but the obſerver need only make the image of the mark coincide with the direct object, then turn himſelf half round, and now taking the mark for the direct object cauſe the image of the former direct object to coincide with the mark, the divided arch of the quadrant being kept upwards, and the place of the centre of the quadrant remaining alſo the ſame in both caſes: half the difference of the ſum of the two inſtrumental angles from 360 degrees will be the correction of the adjuſtment as before.

Should only one of the objects be near, and the other remote (that is to ſay, half a mile diſtant or more) let the diſtant object be taken for the direct one, and the near object for the reflected one; and the true diſtance of the objects as ſeen from the centre of the index-glaſs will be obtained without requiring any correction, whether it be the back or fore obſervation that is made uſe of; only obſerving, as uſual, to take the ſupplement of what is ſhewn upon the arch to 180 degrees in the back-obſervation.

XV. *Account*

XV. *Account of the Irruption of* Solway Mofs *in* December 16, 1772; *in a Letter from Mr.* John Walker, *to the Earl of* Bute, *and communicated by his Lordſhip to the Royal Society*.*

My Lord,

Read Feb. 13, 1772.

WHEN I was ſitting yeſterday writing to your Lordſhip, I received the honour of yours. I ſhall therefore defer the account I intended of my expedition laſt ſeaſon to the north, and give the beſt deſcription I can, of the extraordinary irruption of Solway-moſs, which I went to viſit, about a week after it happened.

It is not ſurprizing, that it has every where attracted the attention of the public; for though the cauſe of it is obvious, yet ſo far as I recollect, the alteration it has produced on the face of the earth, is greater than any we have known in Britain, from natural cauſes, ſince the deſtruction of Earl Goodwin's eſtate.

* The Society has received, from other hands, ſeveral accounts of this curious and ſingular phænomenon; but this, as one of the lateſt, being likewiſe the completeſt, was thought the moſt proper to be laid before the public; eſpecially as, on compariſon, few particulars of any importance mentioned in the other accounts were found wanting in this. Theſe few, however, have been collected, and ſubjoined in the form of notes. M. M.

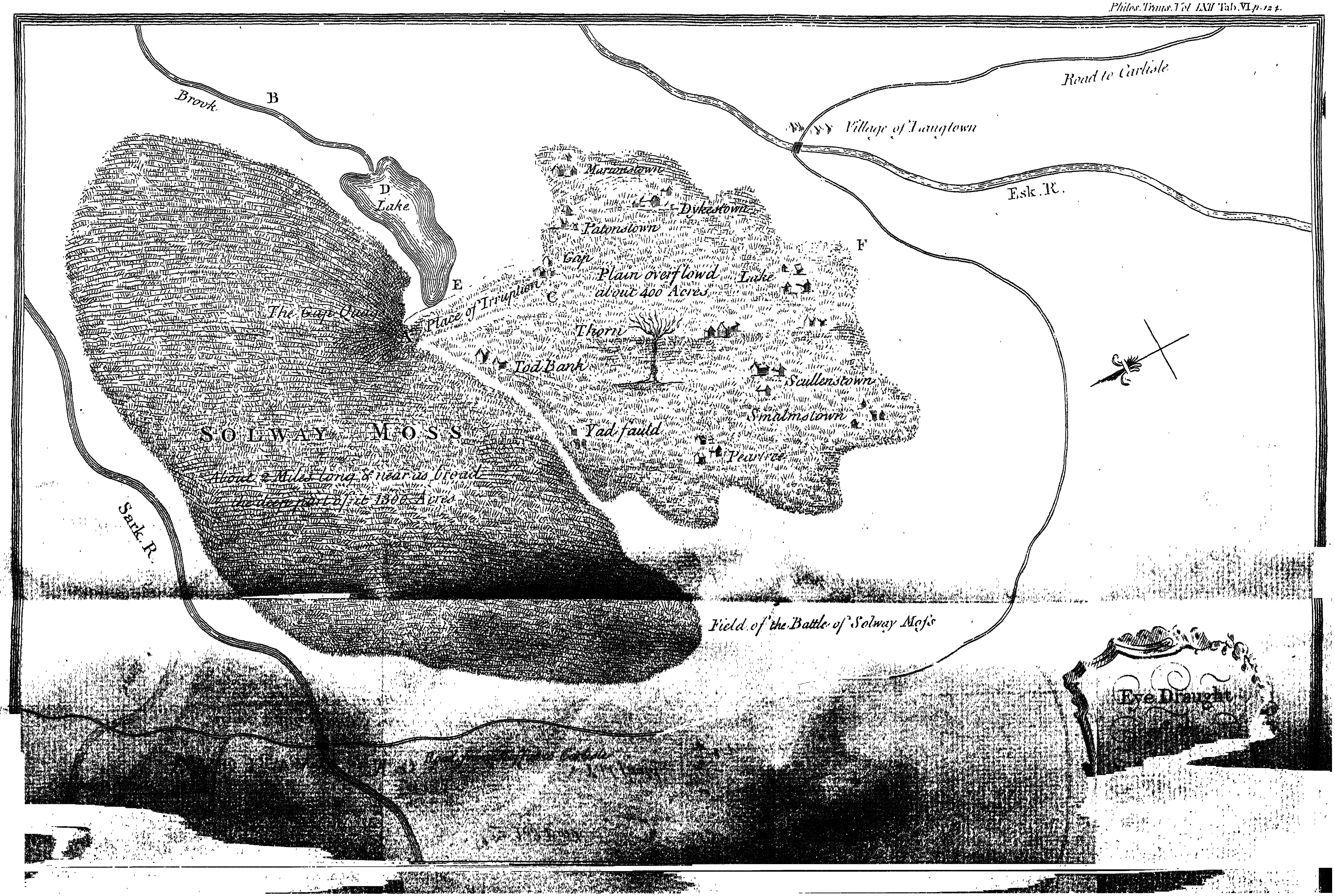
It

It happened on the 16th of December, when there fell ſuch a deluge of rain, over all the North of England, as has not been known, for at leaſt two hundred years. There was a very great flood at Moffat, but I think, I have ſeen one or two greater, and certainly it was not ſo extraordinary here, as further South.

The Solway flow contains 1300 acres of very deep and tender moſs, which, before this accident, were impaſſable, even in ſummer, to a foot paſſenger. It was moſtly of the quag kind, which is a ſort of moſs covered at top with a turf of heath and coarſe aquatic graſſes; but ſo ſoft and watery below, that, if a pole is once thruſt through the turf, it can eaſily be puſhed, though perhaps 15 or 20 feet long, to the bottom. If a perſon ventures on one of theſe quags, it bends in waves under his feet; and if the ſurface breaks, he is in danger of ſinking to the bottom *. The ſurface of the flow was, at different places, between 50 and 80 feet higher than the fine fertile plain, that lay between it and the river Eſk. See Tab. VI. About the middle of the flow, at the place marked A, were the deepeſt quags, and there the moſs was elevated higher above the plain, than in any part of the neighbourhood. From this, to the farm called the Gap, upon the plain at C, there was a broad gully,

* The ſurface was always ſo much of a quagmire, that, in moſt places, it was hardly ſafe for any thing heavier than a ſportsman to venture upon it, even in the drieſt ſummers. A great number of Scotchmen, in the army commanded by Oliver Sinclair in the time of Henry VIII. loſt their lives in it; and it is ſaid that ſome people digging peats upon it, met with the ſkeleton of a trooper and his horſe in compleat armour, not many years ago.

 though


Brook
B
Road to Carlisle
Village of Langtown
D
Lake
Esk. R.
Dykestown
Patonstown
F
Gap
Plain overflowd
about 400 Acres
Lake
E
C
Place of Irruption
A
Thorn
Tod Bank
Scullenstown
Smalmstown
SOLWAY MOSS
Yad fauld
Peartree
About 2 Miles long & near as broad
the deep part of it 1300 Acres
Sark R.
Field of the Battle of Solway Moss
Eye Draught

though not very deep, through which the brook marked B ufed to run. The mofs being quite overcharged with the flood, burft at thefe quags, about 11 o'clock at night, and finding a defcent at hand, poured its contents through the gully into the plain.

It furprized the inhabitants of 12 towns in their beds *. Nobody was loft, but many of the people faved their lives with great difficulty. Next morning, thirty-five families were found difpoffeffed, with the lofs of moft of their corn and fome cattle †. Some of the houfes were near totally covered, and others of them I faw ftanding in the mofs, up to the thatch, the fide walls being about 8 feet high.

In the morning, above 200 acres were entirely overwhelmed; and this body of mofs and water, which was of fuch a confiftency, as to move freely, continued to fpread itfelf on all hands, for feveral days. It was come to a ftop, when I faw it, and had covered 303 acres, as I was informed by a gentleman, who had looked over the plans of the grounds, with Mr. Graham the proprietor: but every fall of rain fets it again in motion, and it has now overfpread above 400 acres. At F, it had run within a mufket fhot of the poft road leading from

* Thofe who were neareft the place of burfting were alarmed with the unufual noife it made; others not till it had entered their houfes, or even, as was the cafe with fome, not till they found it in their beds.

† The cafe of a cow feems fingular enough to deferve a particular mention. She was the only one of eight in the fame cow-houfe, that was faved, after having ftood fixty hours up to the neck in mud and weter. When fhe was got out, fhe did not refufe to eat, but water fhe would not tafte, nor could even look at, without fhewing manifeft figns of horror. She is now reconciled to it, and likely to recover.

Moffat

Moffat to Carlifle, when I faw it, but it is now flowed over the road, and reached the Efk. This river, which was one of the cleareft in the world, is now rendered black as ink, by the mixture of the mofs, and no falmon has fince entered into it. A farmer alfo told me, that, upon removing the mofs, to get at a well which it had covered, they found all the earth-worms lying dead upon the furface of the ground. The land, that is covered, was all inclofed with hedges, bore excellent crops of wheat and turnips, and rented from between 11 and 14 fhillings, befides the taxes and tithes, which amounted to 4 fhillings per acre.

I endeavoured to guefs at the depth of the mofs upon the plain, by a large thorn, which ftands in the middle of it, and which is buried to above the divifion of the branches. The farmers told me, that it ftood upon a rifing, more than 6 feet above the general level of the plain; and that it was upwards of 9 feet high, of clear ftem. By this account, great part of the plain muft be covered 15 feet deep with the mofs: and near the farm called Gap, there were fome confiderable hollows, where they think the mofs, at prefent, lies full 30 feet deep. The talleft hedges on the land are all covered over the top. The houfes are not fo much buried, becaufe they ftood moftly on the higher parts of the fields; and, towards the extremities of the mofs, I obferved it, in many places, not above 3 or 4 feet deep, owing likewife to the rifing of the ground.

The gut at A, through which the whole of the mofs flowed that covered the plain, is only about

50 yards wide, and the gully from A to C is near a quarter of a meaſured mile long.

The brook B, being ſtopped up by the moſs at E, has now formed a lake at D.

About 400 acres of the flow, next the place of its evacuation, appear to have ſunk from 5 to 25 feet: and this ſubſidence has occaſioned great fiſſures upon thoſe parts of the moſs which refuſed to ſink. Theſe fiſſures are from 4 to 8 feet wide, and as much in depth. The ſurface of the flow, conſiſting of heath and coarſe graſs, was torn away in large pieces, which ſtill lie upon the ſurface of the new moſs, ſome of them from 20 to 50 feet long. But the greater part of the ſurface of the flow remained, and only ſubſided; the moſs, rendered thin by the flood, running away from under it.

Looking over the Solway moſs, at the village of Longtown, where there is a bridge on the Eſk, they formerly ſaw only the tops of the trees at Gratney, a houſe of the Marquis of Annandale's, 4 miles diſtant; but now they ſee them almoſt to the ground. And looking over it, in another direction, they now ſee two farm-towns of Sir William Maxwel's, which were not before viſible. So that the ridge of the flow or moſs ſeems to have ſubſided about 25 feet.

I ever am, with the higheſt reſpect,

My Lord,

Your Lordſhip's moſt obliged

and devoted ſervant,

Moffat,
Jan. 30, 1772.

John Walker.

XVI. A

XVI. *A Letter from* John Zephaniah Holwel, *Esq; F. R. S. to* John Campbel, *Esq; F. R. S. giving an Account of a new Species of Oak.*

Exeter, Feb. 24, 1772.

S I R,

Read April 1, 1772.

IN my curious rambles through the environs of this city, I have been tempted to viſit the nurſery of Mr. William Lucombe, of St. Thomas, on the report of a very extraordinary and new ſpecies of oak, firſt diſcovered and propagated by that ingenious gardener; and as this plant appears to me capable of proving an ineſtimable acquiſition to this kingdom, I cannot reſiſt the deſire I feel of communicating to you ſome particulars relative to its hiſtory and character, taken partly from Mr. Lucombe's account of it, and my own obſervations. This, I know, muſt be moſt acceptable to you, who are ſo laboriouſly and laudably employed in elucidating the various improvements and advantages your country is capable of.

About ſeven years paſt, Mr. Lucombe ſowed a parcel of acorns, ſaved from a tree of his own growth, of the iron or wainſcot ſpecies; when they came up, he obſerved one amongſt them that kept

his

his leaves throughout the winter; ſtruck with the phænomenon, he cheriſhed, and paid particular attention to it, and propagated, by grafting, ſome thouſands from it, which I had the pleaſure of ſeeing, eight days ago, in high flouriſhing beauty and verdure, notwithſtanding the ſeverity of the winter. Its growth is ſtraight, and handſome as a fir, its leaves ever-green, and the wood is thought, by the beſt judges, in hardneſs and ſtrength to exceed all other oak. He makes but one ſhoot in the year, viz. in May, and continues growing without interruption; whereas other oaks ſhoot twice, namely, in May and Auguſt; but the peculiar and eſtimable part of its character is, the amazing quickneſs of its growth, which I imagine may be attributed (in ſome degree at leaſt) to its making but one ſhoot in the year; for I believe all trees that ſhoot twice, are, for ſome time, at a ſtand before they make the ſecond. I had the curioſity to take the dimenſions of the parent tree (ſeven years old), and ſome of the grafts; the firſt meaſured 21 feet high, and full 20 inches in the girt; a graft of four years old 16 feet high, and full 14 inches in the girt; the firſt he grafted is ſix years old, and has out-ſhot his parent 2 feet in heighth. The parent tree ſeems to promiſe his acorns ſoon, as he bloſſoms, and forms his foot-ſtalk ſtrong, and the cup upon the foot-ſtalk with the appearance of the acorn, which, with a little more age, will ſwell to perfection. This oak is diſtinguiſhed, in this county, by the title of The Lucombe Oak; his ſhoots, in general, are from 4 to 5 feet every year, ſo that he will, in the ſpace of thirty or forty years, out-grow in altitude and girt the common oak at a

 hundred.

hundred. In two or three days I will forward to you, in a parcel, a branch, which I cut off from the original tree, and another from the graft of four years old, alſo a dead branch of the iron or wainſcot oak, juſt to ſhew that, from the ſimilarity of the leaves, it is a deſcendant from that ſpecies, although differing from it in every other particular. I ſend you alſo, by the Exeter ſtage, a ſpecimen of the wood. I have a walking-pole full 5 feet long, a ſide ſhoot from one of the grafts, only one year and half old. Several gentlemen round this neighbourhood, and in the adjoining counties of Cornwall and Somerſet, have planted them, and they are found to flouriſh in all ſoils.

I am, Dear Sir,

Your faithful friend, and

moſt obedient humble ſervant,

J. Z. Holwell.

XVII. *An Account of the Death of a Person destroyed by Lightning in the Chapel in* Tottenham-Court-Road, *and its Effects on the Building; as observed by Mr.* William Henly, *Mr.* Edward Nairne, *and Mr.* William Jones. *The Account written by Mr.* Henly.

March 24, 1772.

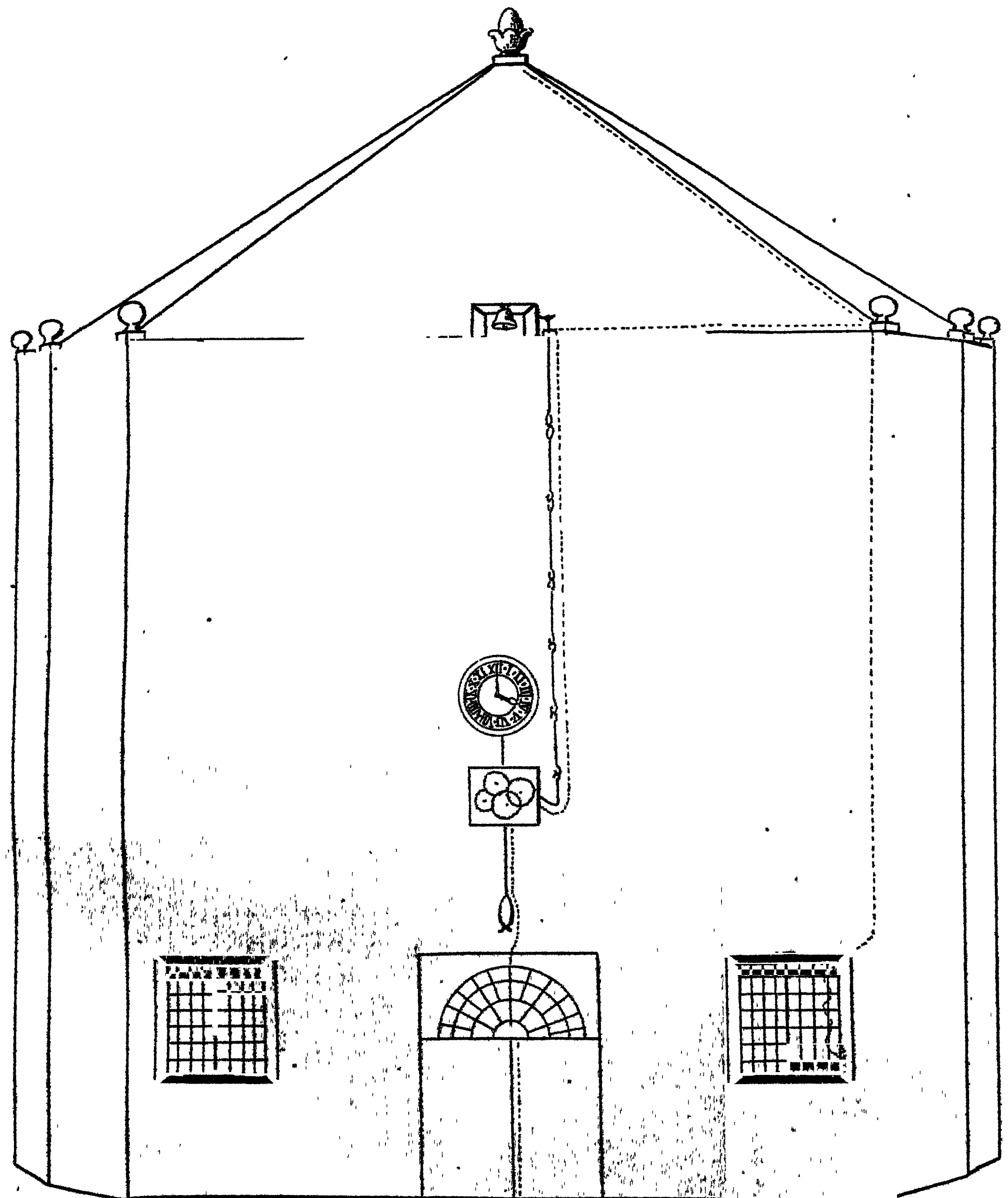
Read April 9, 1772.

ON Sunday last, exactly at 4 o'clock, P. M. part of a building erected by the late Rev. Mr. Whitfield, in Tottenham-court-road, commonly called the Chapel or Tabernacle, was struck by a flash of lightning. This part was an addition afterwards made to the original structure, but was greatly inferior to it in height. On its summit stood an ornament representing a pine-apple carved in wood, which consisted of two pieces; the uppermost being connected with the lower by means of several iron spikes. It was supported by a strong plinth of wood covered with lead lapped over the edges and corners of its top, and there secured by large iron nails. This lead work was connected with that which covered the hips, and made a regular communication of metal, to the bottom of the slating, where it united with a leaden gutter which extended

quite round the building. In this gutter was erected a ſmall lanthorn, in which hung the bell of the clock. A little pipe of lead was ſoldered to, and extended perpendicularly a few inches above, the ſurface of the gutter; through this pipe went a ſmall iron wire conſiſting of many long links, connected with the tail of the hammer; paſſing thence within a few inches of the ſtriking rod of the clock, to which it was tied by a ſtrong hempen ſtring 6 inches or more in length. The lightning firſt ſtruck the pine-apple, the upper part of which it ſhivered into very ſmall fragments, and threw them in all directions from the place, and melted off the end of one of the ſpikes. It left a ſmoky track upon the under-part of it, and then ſtruck the edge of the lead upon the plinth, which it melted in two places, quite through the ſubſtance of it. A little below theſe I found a third ſpot; this was melted in a very regular and curious concave about an $\frac{1}{8}$th of an inch diameter at the ſurface, with a ſmall perforation at the bottom, through which I think might have been introduced one of the fineſt ſort of ſewing needles. The whole figure ſomewhat reſembled a ſmall funnel *. It paſſed thence by a regular communication of metal, till it reached the wire of the clock hammer before ſpoken of, melting it about half through its diameter, which, in this place, was leſs than the twelfth part of an inch. The edge of the lead pipe from which it leaped to the wire was alſo much melted. The wire was melted at every juncture of the links; the packthread at the bottom was but little injured, but the electric matter leaped through

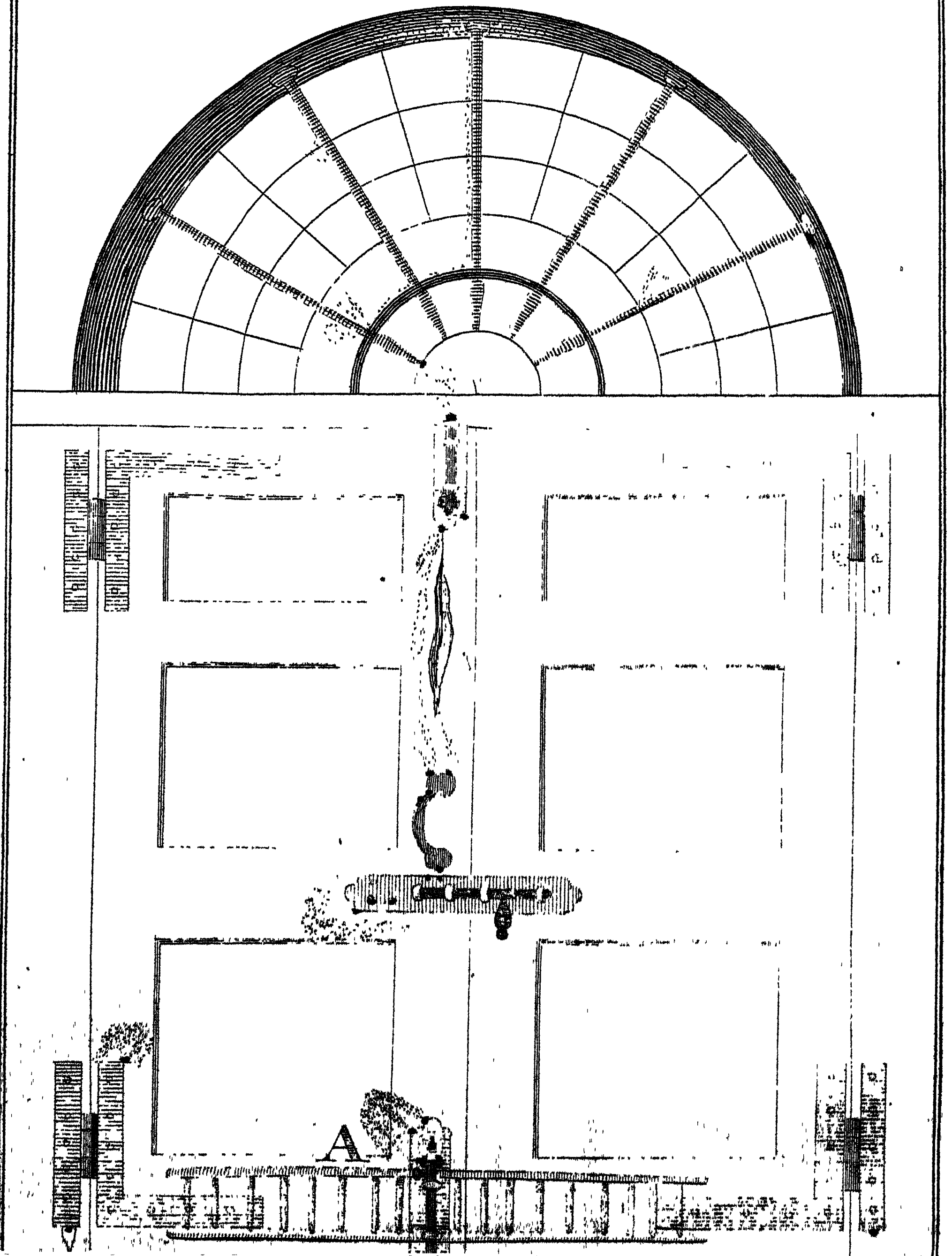
* Quere, is not this a token of the ſtroke's being from the clouds downwards?

a few



An outside view of that part of the Building which was affected by the Lightning.

N.B. The dotted Line represents the course the Electricity took in its passage to the [illegible]



An inside view of the Door & Window.

The dots on the Iron-work & the fine strokes represent the marks left at those places by the Lightning. The person destroy'd sat on the Ladder at A.

Basire sc.

a few inches of air to the ſtriking rod of the clock, in which, near the end, it melted a large ſpot, whence it was conducted by the work of the clock to the upper part of the pendulum, in the axis of which it melted another large ſpot, and deſcended by the rod paſſing over the ball, which it melted in a moſt remarkable manner in ſix or ſeven places (perhaps upon the ball it might accumulate, and, for want of a proper conveyance, break out in different parts of it) and quitted it at the bottom of the nut, which is melted in three places, and will accompany this paper. Here the electricity leaped through eight inches of air, or paſſed in conductors of the worſt kind, dry brick and wood (with a conſiderable cavity between them), till it reached the frame of a window, over the doors, where it broke the ceiling, and burnt the wood to a coal. Here it met with the point of a nail, driven upward into the window frame as a ſecurity to the center bar. The point of this nail is melted off, I ſuppoſe, full half an inch; it was alſo melted in two large ſpots on the oppoſite ſides near the head. My friend Mr. Jones drew it from the bar, &c. This gentleman was alſo ſo obliging as to take down a ſketch of the window, and an outline of the parts affected of the building. [See Tab. VII.] The nail is now in the cuſtody of Mr. Nairne. The lightning paſſed down the aforementioned bar, and by a bent iron (in contact with both), into another bar, whoſe point (which was greatly melted) came much nearer the upper bolt of the door. The lead-work, from the point of the bar was melted, and a board nearly in contact with the ſtaple of the bolt much blacked by the paſſing of the electricity.

Here

Here it ſtruck the upper edge of the ſtaple, which projected a little above the top of the bolt, melted it in a moſt extraordinary manner; the ſpot, and indeed ſeveral others, having run into a kind of ſpiral form, which is raiſed conſiderably, as may be plainly diſtinguiſhed by a very ſhallow magnifier, and often, as in this, by the naked eye. This effect was firſt obſerved by Mr. Nairne. When it quitted this bolt, it ſtruck upon a ſemicircular handle of iron (firſt tearing out a large piece from the door), the upper part of which hath three melted ſpots, beſides a ſingle one at the upper edge of it. But, in quitting it, the electricity melted only one ſpot at the lower edge *, which I think, as Mr. Bell (a gentleman who was with us) obſerved, was a criterion whereby to judge of the direction of the fluid. To the left of this door, at the diſtance of eleven feet four inches, came down a leaden pipe, which terminated at the ceiling, and there juſt entered a pitched trunk of fir (which indeed was the caſe with every leaden pipe about the building). Here the lightning exploded, rending the trunk, and doing other ſlight damage in and about a window, to which it was attracted by an interrupted and irregular communication of metal. I would beg leave to remark, that, had this pipe of lead been continued to the bottom of the building, and thence conveyed into the earth, in the manner directed by Dr. Franklin, I can have no doubt but the whole contents of the exploſion would have paſſed this way, have been

* Quere, is not this effect ſomewhat analogous to Mr. Lullin's electrical experiment with a card?

 conducted

conducted with perfect ſafety to the building, &c. and that no other part of it would have been at all affected. As the effects of this ſtroke ſo exactly correſpond with thoſe many times before obſerved by Dr. Franklin, I think we ſhall ſcarce ever meet with a greater proof of the utility of his metallick conductors; and cannot help expreſſing a ſincere wiſh, that builders, and perſons engaged in the erection of public edifices, &c. might be prevailed with, to make a regular communication of metal, from the top of ſuch buildings to a conſiderable depth into the earth, and of ſuch a diameter and kind, as may be ſufficient to ſecure both the buildings, and the lives of thoſe, who may happen to be in them. The poor man deſtroyed by this accident, was ſitting at the time on a ſhort ladder, which lay horizontally on the pavement, with his back againſt the door. The lightning flew from the middle bolt, and ſtruck him on and under his left ear, entered his neck, making a wound half an inch long, raiſed in a bur and burnt, paſſed down his back, which it turned black as ink, down his left arm, melting the ſtud in his ſhirt ſleeve; the ſtone in which, as well as the ſilver, ſeems to be a little affected. Hence it flew into his body, which it burnt in a hard ſpot, reſembling ſcorched leather, paſſing through it into his right leg, and breaking out a little above the ancle; making a large wound, and another bur, burnt as before, with two others ſmaller a little below it, and ſome ſtill ſmaller in his feet. His cloaths and hair were much burnt, but his ſtock, ſhoe, and knee-buckles, the metal buttons on his coat and waiſtcoat, a ſhilling, which he had in the

left

left pocket of his breeches, and the metal clafps of a Common Prayer-Book, in his coat-pocket, were all uninjured *. His death was truly inftantaneous; he hath left a widow and two children in diftreffed circumftances, who were entirely dependent on his labour. His name was Goodfon, aged thirty-four, by trade a taylor, at N° 3. Craven-Buildings, Drury-Lane.

P. S. The ftuds above-mentioned will be fent together with this paper (as a curiofity) for the infpection of the members of the Royal Society.

* The corps, after lying two or three days on a table, feemed not more difpofed to putrefaction, than bodies at that time generally are, which die a natural death.

XVIII. *A Letter from* Thomas Ronayne, *Esq; to* Benjamin Franklin, *LL. D. F. R. S. inclosing an Account of some Observations on Atmospherical Electricity; in regard of Fogs, Mists, &c. with some Remarks; communicated by Mr.* William Henley.

S I R,

Read April 30, 1772.

IN conformity to the desire of some friends, I have drawn up the following observations on atmospherical electricity, which I beg leave to lay before you; and shall think the trouble I have had, in prosecuting the necessary experiments, sufficiently compensated, if it shall appear to you that they contain any thing new or curious; in which case, you are at liberty to dispose of them in whatever manner you shall think proper.

I am, SIR,

With very respectful consideration,

Your most obedient servant,

Cecil-Street,
Feb. 15, 1772.

Thos Ronayne.

* SOME years ago I discovered, by Mr. Canton's electrometer, described in the Philos. Transactions, Vol. XLVIII. p. 783. that the air of Ireland is, during the winter season, in almost a constant state of positive electricity; which, however, is so weak, that, in order to observe it satisfactorily, I have always found it necessary to have the cork-balls suspended from threads of a middling fineness, six or seven inches in length, quite streight, and to avoid, as much as possible, any interruption from the wind.

I have likewise had frequent recourse to the following contrivance, by which I was enabled, within doors, to pursue my inquiries with greater accuracy and advantage: having procured a slender tapering piece of wood, about five feet long, to the smaller end of which an electrometer was affixed, by means of a small hook; I placed it out from an open garret window, and fastened the other end with a small hasp to one of the jambs: I had also at hand another piece of wood, in the ends of which, a small glass tube and a stick of sealing-wax had been inserted. Either of these was occasionally excited, and applied near the cork-balls, in order to determine more precisely the kind of electricity with which they might happen to be affected; and I was always careful in

* I commenced my experiments on the air in the year 1761, and those on the clouds in the year 1762. I continued them unremittingly, as opportunity offered, till the year 1770, when my occasions brought me to England. I have repeated my experiments, with regard to the common air, in different places here, and find it the same as that in Ireland.

making

making my experiments on that ſide of the houſe where the wind had leaſt power.

I have found the air, in winter, at a proper diſtance from buildings, trees, maſts of ſhips, &c. very ſenſibly electrified, during a froſty or foggy ſtate of the weather; and in miſts too, but in a leſs degree: I have alſo diſcovered ſmall ſigns of it in calm and cloudy weather.

The air, in ſummer, never ſhewed any ſign of electricity, except when a fog happened in the cool of the evening, or at night; in which caſe, I always diſcovered manifeſt marks of electricity, ſenſibly weaker than thoſe obſerved in winter fogs, but preciſely of the ſame kind, that is, *poſitive*.

I have often examined the ſtate of the air, at the time of an Aurora borealis, and could not diſcover any indication of electricity, except when a fog had appeared at the ſame time; in which caſe, the electricity has been, in every reſpect, the ſame as that of a fog at any other time. Once, indeed, during an Aurora borealis on a remarkable ſerene night, I diſcovered ſome ſigns of a very weak *poſitive* electricity.

As the electricity of the air is generally *poſitive* (I never knew an exception but one, which preſented itſelf during a fog on a winter day, that proved uncommonly warm), is it not reaſonable to believe, that cold electrifies the atmoſphere *poſitively*? and, if ſo, may not one be led to imagine, that heat electrifies it *negatively*? But this I only offer as a conjecture, not being able to advance any thing deciſive on the ſubject, and knowing that one ſort of electricity may

often be productive of the other, as is plain from Dr. Franklin's experiments.

If cold electrifies the air *positively* in this climate (which seems extremely probable), may it not electrify it *negatively* at and about the place of our antipodes? Does not a consideration of the effects discovered in the Tourmalin favour this surmise?

The electricity of the air, in frosty, foggy or misty weather, is not strong enough to yield any spark, even by insulating a sharp pointed wire in it, which, however, attracts very light bodies at a small distance; whilst, on the contrary, that of the clouds generally affords considerably strong sparks.

When a fog becomes very thick, the cork-balls approach; but when it returns to its former state, they open again at their first distance; and I have observed that, when it rained in foggy weather, the balls closed, and opened again on the fog's appearance anew, after the rain had ceased: there is, however, a certain degree of density necessary in a fog, in order that the balls might exert their greatest divergency.

Most, if not all, fogs partake of a smell much like that of an excited glass tube, and, indeed, so does the common air very frequently.

As fogs sometimes appear in a very moist state of the air, I was for some time at a loss to account on what principle they could retain their electricity; but having at length remarked, that electrified bodies, insulated with sealing-wax, preserved their electricity for a time in very damp air, I concluded that moisture is but a very slow conductor.

Having,

Having, on the contrary, obſerved that bodies, inſulated with dried ſilk, had loſt their electricity in a very ſhort time, I attempted to render it a non-conductor, by having varniſhed it over with oil of turpentine, balſam of ſulphur, and ſuch like, but did not ſucceed; for ſilks ſo treated ſoon became a conductor, and increaſed conſiderably in weight, if the air happened not to be very * dry; ſo much indeed, that I think ordinary ſilk, from its power of abſorbing moiſture from the air, may well ſerve as an occaſional hygrometer, either by being put into a balance, or by having an electrified body inſulated with it.

When the denſity of fogs, floating near the earth, increaſes conſiderably, the balls always approach; but when they are ſituated high in air, the reverſe generally happens. I had an opportunity of remarking a ſtruggle between breezes from the north-weſt and ſouth-eaſt at the ſame time, in which the one ſeemed ſometimes to prevail, and afterwards the other. This contention was ſucceeded by a ſmoaky hazineſs, which, like a fog, occaſioned the balls to open: as the hazineſs † thickened, they opened

* Even glaſs attracts moiſture to its ſurface, which makes it a conductor of electricity, and, conſequently, not ſo convenient as ſealing-wax.

† An electrical body, when contracted in its dimenſion, will have its electricity increaſed, as appears by Dr. Franklin's curious experiment with the chain and ſilver can. I alſo have diſcovered, from repeated trials, that a piece of flannel, ſilk, &c. excited, and ſuddenly twiſted, not only ſtruck at a greater diſtance than before, but ſometimes emitted pencils of fire into the air. May we not hence infer why the electricity of vapour, &c. (when not in contact with the earth) increaſes by condenſation?

wider,

wider, and ſtill wider when it diſſolved into rain; but their repelling power became greateſt in proportion as the drops increaſed.

The electrometer placed out from a garret window (p. 138.), has been frequently uſeful to me, in determining the nature of an approaching cloud, whoſe electricity, although generally ſtrong, was for the moſt part uncertain, having been ſometimes *poſitive*, and at other times *negative*. But, as the wind or rain were frequent impediments to the accuracy of my experiments, the following methods of making obſervations, with ſucceſs, under ſhelter, occurred to me.

I have ſometimes ſtood, in an upper room, on a cake of wax, holding in my right hand, out at the window, a long ſlender piece of wood, round which a wire projecting a few inches had been twiſted, and in my left hand an electrometer: an aſſiſtant had excited glaſs or wax in readineſs.

At other times, I have made uſe of a tapering tube of tin, twenty feet long, ending in a point; the greateſt part of it ſtood out high in the air, and the thick end, from which an electrometer hung, was ſupported inſide the window, ſometimes with ſilk cords, and at other times with ſtrong ſticks of ſealing-wax, ſuſtained at either end by hooks of iron-wire.

By either of theſe means I have often diſcovered, that what ſeemed to me a ſingle cloud, produced, in its paſſing over, ſeveral ſucceſſive changes, from *poſitive* to *negative*, and from *negative* to *poſitive* electricity, the balls coming together each time, and remaining in contact a few ſeconds, before they repelled each other again.

The

The permanence of either kind of electricity in the clouds, or the length of time in which neither can be discovered, is uncertain; sometimes the same electricity has returned, and at other times has been succeeded by the contrary; whilst either generally came on, and went off gradually. But changes were often made, very suddenly, by a flash of lightning, especially if the thunder-storm happened to be in the zenith. A branch of it, over-head, has frequently occasioned stronger electricity than I could discover, when the greatest part of the sky had been overcast; which, perhaps, might be accounted for, from this consideration, that one kind of electricity acting alone, must exert more powerful effects than when counteracted by the other.

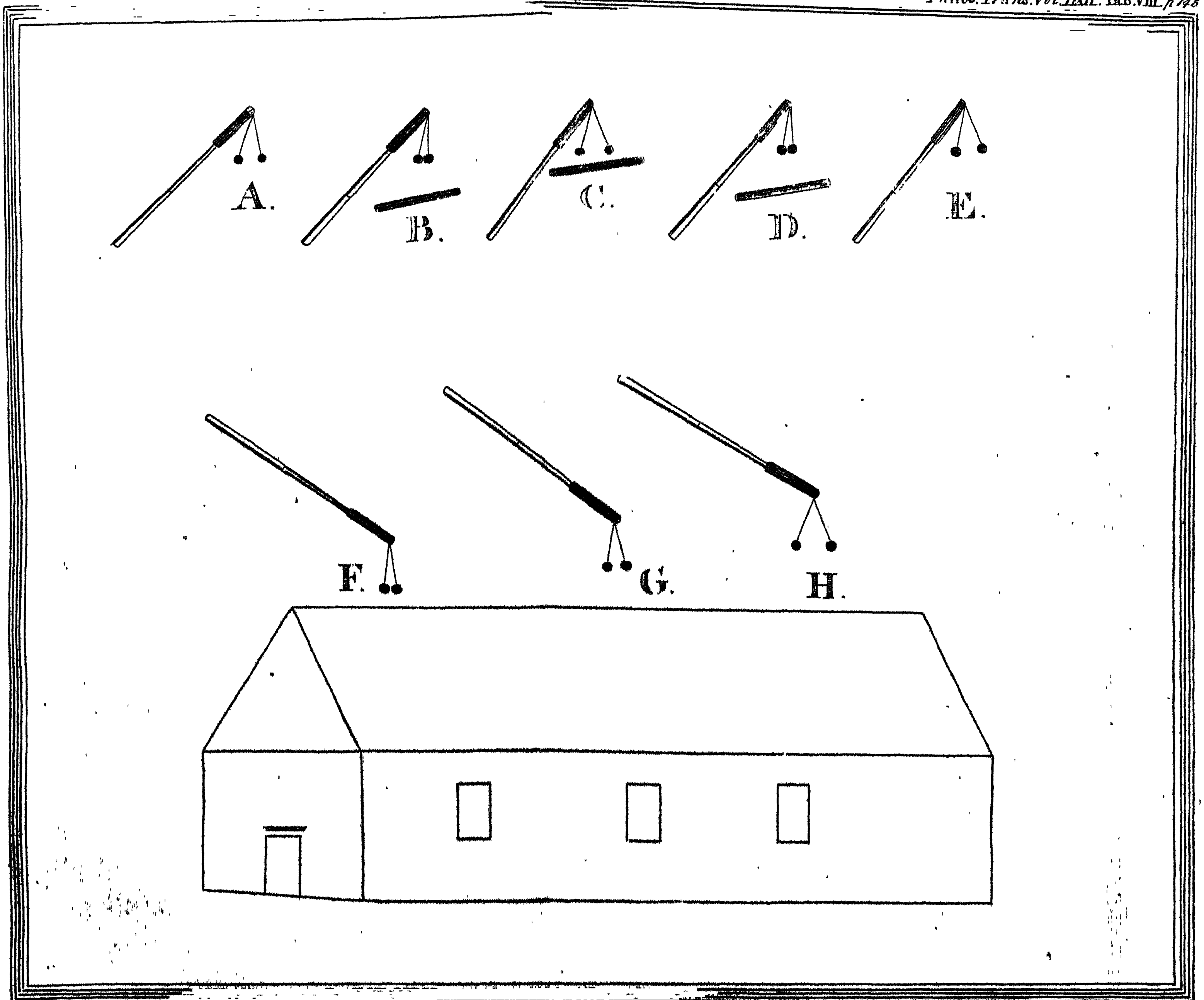
I once observed in a thunder-storm, during which I saw no lightning, that the balls, which hung from the tin tube, repelled and attracted each other, very rapidly, for the space of ten or twelve seconds; at the same time, Mr. Canton's electrometer, which I held at such a distance from the tube, as to have its balls opened to the distance of an inch, continued quiet in that state, and were not affected convulsively like the others. Hence I imagined, that the same kind of electricity went off, and came on, without being changed *in contrarium*; for when that circumstance happened, they were very evidently affected in the same manner. And here I must observe, that I have found it more easy to discover the kind of electricity present in the tube, by approaching excited wax to the balls of an electrometer, which I held at a proper distance from the tube, than by applying it near the balls which hung from the tube; for

for they, in the general, diverged ſo much, that I found it very difficult to have in readineſs a ſmall tube of glaſs, or wax ſufficiently excited to affect them.

It has ſometimes happened that the balls of the tin tube, &c. perfectly at reſt, have, in conſequence of a flaſh of lightning, ſuddenly repelled each other, and immediately after cloſed. As this circumſtance has frequently happened, when the air was in a damp ſtate, I have ſometimes imagined that the equilibrium between the earth and lower clouds had been quickly reſtored, on receiving the electricity of the higher ones; and, at other times, have ſuppoſed that it might be owing to the lateral effect of the exploſion.

If two or more perſons, at a ſufficient diſtance from each other, would correſpond, by ſignals, viz. a red flag for *poſitive*, and a blue one for *negative* electricity, we ſhould probably obtain, in due time, more ſatisfactory certainty with regard to the electricity of the clouds, thunder, &c. than has hitherto been given, or is, perhaps, poſſible for any one man to acquire, without the aid of wires or chains, produced from different apparatuſſes, placed at different diſtances from each other.

Mr.



Basire sc.

Mr. Ronayne having received the following Letter from Mr. Henly, which corroborates and confirms the obſervations mentioned in his paper, it was thought proper to print them together in this volume.

October 16, 1771, ¼ paſt 5, P. M.

I Find a fog (not very thick), ſoon after its appearance, ſtrongly electrical. The balls open ½ or ¾ inch A, [See Tab. VIII.], and cloſe at the approach of excited wax, when brought within 10 inches of them B: if the wax is brought within 3 or 4 inches, they diverge again, in conſequence thereof C: as the wax is withdrawn, they converge again, D, till it gets beyond the diſtance of its influence, when they begin to diverge again; and, as the wax is withdrawn ſtill farther, they continue to open, in conſequence of the electricity in the fog, till they reach their original diſtance from each other E. There is very little diſturbance by the wind, and the little there is, only wafts them in a ſmall degree, but they keep ſeparate. If they are held near the tiling, or brick-work, of a neighbouring houſe, they cloſe, F; but begin to diverge again, at the diſtance of 3 or 4 feet from it, G; and their divergence increaſes, as they recede from the building, till they ſeparate ½ or ¾ inch, as at firſt, H.

 Memo-

MEMORANDUM.

October 3, 1771, I tried the electricity of a thick fog, and (in at least twenty different trials) found the balls separated from $\frac{1}{4}$ to $\frac{3}{4}$ inch distance. Whenever I brought them near the building, or approached them with a stick of excited wax, they closed; and opened again, on removing them.

W. Henly.

XIX. *Obſervations on different Kinds of Air. By* Joſeph Prieſtley, *LL. D. F. R. S.*

Read March 5, 12, 19, 26, 1772.

THE following obſervations on the properties of ſeveral different kinds of air, I am ſenſible, are very imperfect, and ſome of the courſes of experiments are incomplete; but a conſiderable number of facts, which appear to me to be new and important, are ſufficiently aſcertained; and I am willing to hope, that when philoſophers in general are apprized of them, ſome perſons may be able to purſue them to more advantage than myſelf. I therefore think it my duty to give this Society an account of the progreſs I have been able to make; and I ſhall not fail to communicate any farther lights that may occur to me, whenever I reſume theſe inquiries.

In writing upon this ſubject, I find myſelf at a loſs for proper terms, by which to diſtinguiſh the different kinds of air. Thoſe which have hitherto obtained are by no means ſufficiently characteriſtic, or diſtinct. The terms in common uſe are, fixed air, mephitic, and inflammable. The laſt, indeed, ſufficiently characterizes and diſtinguiſhes that kind of air which takes fire, and explodes on the approach of flame; but it might have been termed fixed with

as much propriety as that to which Dr. Black and others have given that denomination, ſince it is originally part of ſome ſolid ſubſtance, and exiſts in an unelaſtic ſtate, and therefore may be alſo called factitious. The term mephitic is equally applicable to what is called fixed air, to that which is inflammable, and to many other kinds; ſince they are equally noxious, when breathed by animals. Rather, however, than to introduce new terms, or change the ſignification of old ones, I ſhall uſe the term fixed air, in the ſenſe in which it is now commonly uſed, and diſtinguiſh the other kinds by their properties, or ſome other periphraſis. I ſhall be under a neceſſity, however, of giving a name to one ſpecies of air, to which no name was given before.

Of FIXED AIR.

Fixed air is that which is expelled by heat from lime, and other calcareous ſubſtances, and, when deprived of which, they become quick-lime. It is alſo contained in alkaline ſalts, and is generated in great quantities from fermenting vegetables; and being united with water, gives it the principal properties of Pyrmont-water. This kind of air is alſo well known to be fatal to animals; and Dr. Macbride has demonſtrated, that it checks or prevents putrefaction.

Living for ſome time in the neighbourhood of a public brewery, I was induced to make a few experiments on this kind of air, there being always a large body of it, ready formed, upon the ſurface of the fermenting liquor, generally about nine inches

or

or a foot in depth, within which any kind of ſubſtance may be very conveniently placed; and though it muſt be continually mixing with the common air, and is far from being perfectly pure, yet there is a conſtant ſupply from the fermenting liquor, and it is pure enough for many purpoſes.

A perſon, who is quite a ſtranger to the properties of this kind of air, would be agreeably amuſed with extinguiſhing lighted candles, or chips of wood in it, as it lies upon the ſurface of the fermenting liquor; for the ſmoke readily unites with this kind of air, probably by means of the water which it contains; ſo that very little or none of the ſmoke will eſcape into the open air, which is incumbent upon it. It is remarkable, that the upper ſurface of this ſmoke, floating in the fixed air, is ſmooth, and well defined; whereas the lower ſurface is exceedingly ragged, ſeveral parts hanging down to a conſiderable diſtance within the body of the fixed air, and ſometimes in the form of balls, connected to the upper ſtratum by ſlender threads, as if they were ſuſpended. The ſmoke is alſo apt to form itſelf into broad flakes, parallel to the ſurface of the liquor, and at different diſtances from it, exactly like clouds. Theſe appearances will ſometimes continue above an hour, with very little variation. When this fixed air is very ſtrong, the ſmoke of a ſmall quantity of gunpowder fired in it will be wholly retained by it, no part eſcaping into the common air.

Making an agitation in this air, the ſurface of it, which ſtill continues to be exactly defined, is thrown into the form of waves, which it is very amuſing to look upon; and if, by this agitation, any of the fixed

air

air be thrown over the ſide of the veſſel, the ſmoke, which is mixed with it, will fall to the ground, as if it was ſo much water, the fixed air being heavier than common air.

The red part of burning wood was extinguiſhed in this air, but I could not perceive that a red-hot poker was ſooner cooled in it.

Fixed air does not inſtantly mix with common air. Indeed, if it did, it could not be caught upon the fermenting liquor; for a candle put under a large receiver, and immediately plunged very deep below the ſurface of the fixed air, will burn ſome time. But veſſels with the ſmalleſt orifices, hanging with their mouths downwards in the fixed air, will in time have the common air, which they contain, perfectly mixed with it. When the fermenting liquor is contained in veſſels cloſe covered up, the fixed air is rendered much ſtronger, and then it readily affects the common air which is contiguous to it; ſo that, upon removing the cover, candles held at a conſiderable diſtance above the ſurface will inſtantly go out. I have been told by the workmen, that this will ſometimes be the caſe, when the candles are held more than half a yard above the mouth of the veſſel.

Fixed air unites with the ſmoke of reſin, ſulphur, and other electrical ſubſtances, as well as with the vapour of water; and yet, by holding the wire of a charged phial among theſe fumes, I could not make any electrical atmoſphere, which ſurprized me a good deal, as there was a large body of this ſmoke, and it was ſo confined, that it could not eſcape me. I alſo held ſome oil of vitriol in a glaſs veſſel within the

the fixed air, and by plunging a piece of red hot glaſs into it, raiſed a copious and thick fume. This floated upon the ſurface of the fixed air like other fumes, and continued as long.

Conſidering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeaſt of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and other medicinal mineral waters. Accordingly, I found, that when the ſurface of the water was conſiderable, it always acquired the pleaſant acidulous taſte that Pyrmont water has. The readieſt way of impregnating water with this virtue, in theſe circumſtances, is to take two veſſels, and to keep pouring the water from one into the other, when they are both of them held as near the yeaſt as poſſible; for by this means a great quantity of ſurface is expoſed to the air, and the ſurface is alſo continually changing. In this manner, I have ſometimes, in the ſpace of two or three minutes, made a glaſs of exceedingly pleaſant ſparkling water, which could hardly be diſtinguiſhed from very good Pyrmont.

But the moſt effectual way of impregnating water with fixed air is to put the veſſels which contain the water into glaſs jars, filled with the pureſt fixed air, made by the ſolution of chalk in diluted oil of vitriol, ſtanding in quickſilver. In this manner I have, in about two days, made a quantity of water to imbibe more than an equal bulk of fixed air, ſo that, according to Dr. Brownrigg's experiments, it muſt have been much ſtronger than the beſt imported Pyrmont; for though he made his experiments at the ſpring

head

head, he never found that it contained quite ſo much as half its bulk of this air. If a ſufficient quantity of quickſilver cannot be procured, oil may be uſed with ſufficient advantage, for this purpoſe, as it imbibes the fixed air very ſlowly. Fixed air may be kept in veſſels ſtanding in water for a long time, if they be ſeparated by a partition of oil, about half an inch thick. Pyrmont water made in theſe circumſtances, is little or nothing inferior to that which has ſtood in quickſilver.

The *readieſt* method of preparing this water for uſe is to agitate it ſtrongly with its whole ſurface expoſed to the fixed air. By this means alſo, more than an equal bulk of air may be communicated to a large quantity of water in the ſpace of a few minutes. Eaſy directions for doing this I have publiſhed in a ſmall pamphlet, deſigned originally for the uſe of ſeamen in long voyages, on the preſumption that it might be of uſe for preventing or curing the ſea ſcurvy, equally with wort, which was recommended by Dr. Macbride for this purpoſe, on no other account than its property of generating fixed air, by its fermentation in the ſtomach.

Water thus impregnated with fixed air readily diſſolves iron, as Mr. Lane has diſcovered; ſo that if a quantity of iron filings be put to it, it preſently becomes a ſtrong chalybeate, and of the mildeſt and moſt agreeable kind.

I have recommended the uſe of chalk and oil of vitriol as the cheapeſt, and, upon the whole, the beſt materials for this purpoſe; and whereas ſome perſons had ſuſpected that a quantity of the oil of vitriol was rendered volatile by this proceſs, I examined it by

by all the chemical methods that are in use; but could not find that water thus impregnated contained the leaſt perceivable quantity of the acid.

Mr. Hey, indeed, who aſſiſted me in this examination, found that diſtilled water, impregnated with fixed air, did not mix ſo readily with ſoap as the diſtilled water itſelf; but this was alſo the caſe when the fixed air had paſſed through a long glaſs tube filled with alkaline ſalts, which, it may be ſuppoſed, would have imbibed any of the oil of vitriol that might have been contained in that air *.

It is not improbable but that fixed air itſelf may be of the nature of an acid, though of a weak and peculiar ſort. Mr. Bergman of Upſal, who honoured me with a letter upon the ſubject, calls it the aërial acid, and, among other experiments to prove it to be an acid, he ſays that it changes the blue juice of tourneſole into red.

The heat of boiling water will expell all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

Dr. Percival, who is particularly attentive to every improvement in the medical art, and who has thought ſo well of this impregnation as to preſcribe it in ſeveral caſes, informs me that it ſeems to be much ſtronger, and ſparkles more, like the true Pyrmont water, after it has been kept ſome time. This circumſtance, however, ſhews that, in time, the fixed air is more eaſily diſengaged from the water, and

* An account of Mr. Hey's experiments will be found in the Appendix to theſe papers.

 though,

though, in this ſtate, it may affect the taſte more ſenſibly, it cannot be of ſo much uſe in the ſtomach and bowels, as when the air is more firmly retained by the water, though, in conſequence of it, it be leſs ſenſible to the taſte.

By the proceſs deſcribed in my pamphlet, fixed air may be readily incorporated with wine, beer, and almoſt any other liquor whatever; and when beer, wine, or cyder, is become flat or dead (which is the conſequence of the eſcape of the fixed air they contained). they may be revived by this means; but the delicate and agreeable flavour, or acidulous taſte, communicated by fixed air, and which is very manifeſt in water, can hardly be perceived in wine, or any liquors which have much taſte of their own.

I ſhould think that there can be no doubt, but that water thus impregnated with fixed air muſt have all the medicinal virtues of genuine Pyrmont water; ſince theſe depend upon the fixed air it contains. If the genuine Pyrmont water derives any advantage from its being a natural chalybeate, this may alſo be obtained by providing a common chalybeate water, and uſing it in theſe proceſſes, inſtead of common water.

Having ſucceeded ſo well with this artificial Pyrmont water, I imagined that it might be poſſible to give ice the ſame virtue, eſpecially as cold is known to promote the abſorption of fixed air by water; but in this I found myſelf quite miſtaken. I put ſeveral pieces of ice into a quantity of fixed air, confined by quickſilver, but no part of the air was abſorbed in two days and two nights; but upon bringing it into a place where the ice melted, the air

was

was abſorbed as uſual. I then took a quantity of ſtrong artificial Pyrmont water, and, putting it into a thin glaſs phial, I ſet it in a pot that was filled with ſnow and ſalt. This mixture inſtantly freezing the water that was contiguous to the ſides of the glaſs, the air was diſcharged plentifully, ſo that I catched a conſiderable quantity, in a bladder tied to the mouth of the phial. I alſo took two quantities of the ſame Pyrmont water, and placed one of them where it might freeze, keeping the other in a cold place, but where it would not freeze. This retained its acidulous taſte, though the phial which contained it was not corked; whereas the other, being brought into the ſame place, where the ice melted very ſlowly, had at the ſame time the taſte of common water only. That quantity of water which had been frozen by the mixture of ſnow and ſalt, was almoſt as much like ſnow as ice, ſuch a quantity of air bubbles were contained in it, by which it was prodigiouſly increaſed in bulk.

The preſſure of the atmoſphere aſſiſts very conſiderably in keeping fixed air confined in water; for in an exhauſted receiver, Pyrmont water will abſolutely boil, by the copious diſcharge of its air. This is alſo the reaſon why beer and ale froth ſo much *in vacuo*. I do not doubt, therefore, but that, by the help of a condenſing engine, water might be much more highly impregnated with the virtues of the Pyrmont ſpring, and it would not be difficult to contrive a method of doing it.

The manner in which I made ſeveral experiments to aſcertain the abſorption of fixed air by different fluid ſubſtances was to put the liquid into a diſh,

and holding it within the body of the fixed air at the brewery, to set a glass vessel into it, with its mouth inverted. This glass being necessarily filled with the fixed air, the liquor would rise into it when they were both taken into the common air, if the fixed air was absorbed at all.

Making use of ether in this manner, there was a constant bubbling from under the glass, occasioned by this fluid easily rising in vapour, so that I could not, in this method, determine whether it imbibed the air or not. I concluded, however, that they did incorporate, from a very disagreeable circumstance, which made me desist from making any more experiments of the kind. For all the beer, over which this experiment was made, contracted a peculiar taste, the fixed air impregnated with the ether being, I suppose, again absorbed by the beer. I have also observed, that water which remained a long time within this air has sometimes acquired a very disagreeable taste. At one time it was like tar-water. How this was acquired, I was very desirous of making some experiments to ascertain, but I was discouraged by the fear of injuring the fermenting liquor. It could not come from the fixed air only.

Having imagined that fixed air coagulated the blood in the lungs of animals, and thereby caused instant death; I suffocated a cat in this kind of air, and examining the lungs presently after, found them collapsed and white, having little or no blood in them.

In order to try the effect of this air upon the blood itself, I took a quantity from a fowl just killed, and divided it into two parts, holding one of them within the

the fixed air, and the other in the common air, and obſerved that the former was coagulated much ſooner than the latter. This I could wiſh to have tried again.

Inſects and animals which breathe very little are ſtifled in fixed air, but are not ſoon quite killed in it. Butterflies, and flies of other kinds, will generally become torpid, and ſeemingly dead, after being held a few minutes over the fermenting liquor; but they revive again after being brought into the freſh air. But there are very great varieties with reſpect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large ſtrong frog was much ſwelled, and ſeemed to be nearly dead, after being held about ſix minutes over the fermenting liquor; but it recovered upon being brought into the common air. A ſnail treated in the ſame manner died preſently.

Fixed air is preſently fatal to vegetable life. At leaſt ſprigs of mint, growing in water, and placed over the fermenting liquor, will often become quite dead in one day, or even in a leſs ſpace of time; nor do they recover when they are afterwards brought into the common air. I am told, however, that ſome other plants are much more hardy in this reſpect.

A red roſe, freſh gathered, loſt its redneſs, and became of a purple colour, after being held over the fermenting liquor about twenty-four hours; but the tips of each leaf were much more affected than the reſt of it. Another red roſe turned perfectly white in this ſituation; but various other flowers, of different colours, were very little affected. Theſe experiments

riments were not repeated, as I wiſh they might be done, in pure fixed air, extracted from chalk by means of oil of vitriol.

For every purpoſe, in which it was neceſſary that the fixed air ſhould be as unmixed as poſſible, I generally made it by pouring oil of vitriol upon chalk and water, catching it in a bladder, faſtened to the neck of the phial, in which they were contained, taking care to preſs out all the common air, and alſo the firſt, and ſometimes the ſecond, produce of fixed air; and alſo, by agitation, making it as quickly as I poſſibly could. At other times, I made it paſs from the phial in which it was generated through a glaſs tube, without the intervention of any bladder, which, as I found by experience, will not long make a ſufficient ſeparation between ſeveral kinds of air and common air.

I had once thought that the readieſt method of procuring fixed air, and in ſufficient purity, would be by the ſimple proceſs of burning chalk, or pounded lime-ſtone in a gun-barrel, making it paſs through the ſtem of a tobacco-pipe, or a glaſs tube carefully luted to the orifice of it; and in this manner I find that air is produced in great plenty; but, upon examining it, I found, to my very great ſurprize, that little more than one half of it was fixed air, capable of being abſorbed by water; and that the reſt was inflammable, ſometimes very weakly, but ſometimes pretty highly ſo. Whence this inflammability proceeds, I am not able to determine, the lime or chalk not being ſuppoſed to contain any other than fixed air. I conjecture, however, that it muſt proceed from the iron, and the ſeparation of it

from

from the calx may be promoted by that ſmall quantity of oil of vitriol, which I am informed is contained in chalk, if not in lime-ſtone alſo. But it is an objection to this hypotheſis, that the inflammable air produced in this manner burns blue, and not at all like that which is produced from iron, or any other metal, by means of an acid. It has alſo the ſmell of that kind of inflammable air which is produced from vegetable ſubſtances. Beſides, oil of vitriol without water, will not diſſolve iron; nor can inflammable air be got from it, unleſs the acid be conſiderably diluted; and when I mixed brimſtone with the chalk, neither the quality nor the quantity of the air was changed by it. Indeed no air, or permanently elaſtic vapour, can be got from brimſtone, or any oil.

In the method in which I generally made the fixed air, and indeed always, unleſs the contrary be particularly mentioned, *viz.* by diluted oil of vitriol and chalk, I found by experiment that it was as pure as Mr. Cavendiſh made it. For after it had paſſed through a large body of water in ſmall bubbles, ſtill $\frac{1}{50}$ or $\frac{1}{60}$ part only was not abſorbed by water. In order to try this as expeditiouſly as poſſible, I kept pouring the air from one glaſs veſſel into another, immerſed in a quantity of cold water, in which manner I found by experience, that almoſt any quantity may be reduced as far as poſſible in little more than a quarter of an hour.

At the ſame time that I was trying the purity of my fixed air, I had the curioſity to endeavour to aſcertain whether that part of it which is not miſcible in water, be equally diffuſed through the whole

maſs;

maſs; and, for this purpoſe, I divided a quantity of about a gallon into three parts, the firſt conſiſting of that which was uppermoſt, and the laſt of that which was the loweſt, contiguous to the water; but all theſe parts were reduced in about an equal proportion, by paſſing through the water, ſo that the whole maſs had been of an uniform compoſition. This I have alſo found to be the caſe with ſeveral kinds of air, which will not properly incorporate.

A mouſe will live very well, though a candle will not burn, in the reſiduum of the pureſt fixed air that I can make; and I once made a very large quantity for the ſole purpoſe of this experiment. This, therefore, ſeems to be one inſtance of the generation of genuine common air, though vitiated in ſome degree. It is alſo another proof of the reſiduum of fixed air being, in part at leaſt, common air, that it becomes turbid, and is diminiſhed by the mixture of nitrous air, as will be explained hereafter.

That fixed air only wants ſome addition to make it permanent, and immiſcible with water, if not, in all reſpects, common air, I have been led to conclude, from ſeveral attempts which I once made to mix it with air, in which a quantity of iron filings and brimſtone, made into a paſte with water, had ſtood; for, in ſeveral mixtures of this kind, I imagined that not much more than half of the fixed air could be imbibed by water; but, not being able to repeat the experiment, I conclude that I either deceived myſelf in it, or that I overlooked ſome circumſtance on which the ſucceſs of it depended.

Theſe experiments, however, whether they were fallacious or otherwiſe, induced me to try whether any

any alteration would be made in the constitution of fixed air, by this mixture of iron filings and brimstone. I therefore put a mixture of this kind into a quantity of as pure fixed air as I could make, and confined the whole in quicksilver, lest the water should absorbe it before the effects of the mixture could take place. The consequence was, that the fixed air was diminished, and the quicksilver rose in the vessel, till about the fifth part was occupied by it; and, as near as I could judge, the process went on, in all respects, as if the air in the inside had been common air.

What is most remarkable, in the result of this experiment, is, that the fixed air, into which this mixture had been put, and which had been in part diminished by it, was in part also rendered insoluble in water by this means. I made this experiment four times, with the greatest care, and observed, that in two of them about one sixth, and in the other two about one fourteenth, of the original quantity, was such as could not be absorbed by water, but continued permanently elastic. Lest I should have made any mistake with respect to the purity of the fixed air, the last time that I made the experiment, I set part of the fixed air, which I made use of, in a separate vessel, and found it to be exceedingly pure, so as to be almost wholly absorbed by water; whereas the other part, to which I had put the mixture, was far from being so.

In one of these cases, in which fixed air was made immiscible with water, it appeared to be not very noxious to animals; but in another case, a mouse died in it pretty soon.

 As

As the iron is reduced to a calx by this proceſs, I once concluded, that it is phlogiſton that fixed air wants, to make it common air; and, for any thing I yet know, this may be the caſe, though I am ignorant of the method of combining them; and when I calcined a quantity of lead in fixed air, in the manner which will be deſcribed hereafter, it did not ſeem to have been leſs ſoluble in water than it was before.

II.

On Air in which a candle, or brimstone, has burned out.

It is well known that flame cannot ſubſiſt long without change of air, ſo that the common air is neceſſary to it, except in the caſe of ſubſtances, into the compoſition of which nitre enters; for theſe will burn *in vacuo*, in fixed air, and even under water, as is evident in ſome rockets, which are made for this purpoſe. The quantity of air which even a ſmall flame requires to keep it burning is prodigious. It is generally ſaid, that an ordinary candle conſumes, as it is called, about a gallon in a minute. Conſidering this amazing conſumption of air, by fires of all kinds, volcano's, &c. it becomes a great object of philoſophical inquiry, to aſcertain what change is made in the conſtitution of the air by flame, and to diſcover what proviſion there is in nature for remedying the injury which the atmoſphere receives by this means. Some of the following experiments will, perhaps, be thought to throw a little light upon the ſubject.

The

The diminution of the quantity of air in which a candle, or brimſtone, has burned out, is various; but I imagine that, at a medium, it may be about one fifteenth, or one ſixteenth, of the whole; about one third as much as by animals breathing it as long as they can, by animal or vegetable ſubſtances putrifying in it, by the calcination of metals, or by a mixture of ſteel filings and pounded brimſtone ſtanding in it.

I have ſometimes thought, that flame diſpoſes the common air to depoſit the fixed air it contains; for if any lime-water be expoſed to it, it immediately becomes turbid. This is the caſe, when wax candles, tallow candles, chips of wood, ſpirit of wine, æther, and every other ſubſtance which I have yet tried, except brimſtone, is burned in a cloſe glaſs veſſel, ſtan 'ing in lime-water. This precipitation of fixed air (if this be the caſe) may be owing to ſomething emitted from the burning bodies, which has a ſtronger affinity with the other conſtituent parts of the atmoſphere.

If brimſtone be burned in the ſame circumſtances, the lime-water continues tranſparent, but ſtill there may have been the ſame precipitation of the fixed part of the air; but that, uniting with the lime and the vitriolic acid, it forms a ſelenetic ſalt, which is ſoluble in water. Having evaporated a quantity of water thus impregnated, by burning brimſtone a great number of times over it, a whitiſh powder remained, which had an acid taſte; but repeating the experiment with a quicker evaporation, the powder had no acidity, but was very much like chalk. The burning of brimſtone but once over a

quantity of lime-water, will affect it in such a manner, that breathing into it will not make it turbid, which otherwise it always presently does.

Dr. Hales supposed, that by burning brimstone repeatedly in the same quantity of air, the diminution would continue without end. But this I have frequently tried, and not found to be the case. Indeed, when the ignition has been imperfect in the first instance, a second firing of the same substance will increase the effect of the first, &c. but this progress soon ceases. In many cases of the diminution of air, the effect is not immediately apparent, even when it stands in water; for sometimes the bulk of air will not be much reduced, till it has passed several times through a quantity of water, which has thereby a better opportunity of absorbing that fluid part of the air, which had not been perfectly detached from the rest. I have sometimes found a very great reduction of a mass of air, in consequence of passing but once thorough cold water. If the air has stood in quicksilver, the diminution is generally inconsiderable, till it has undergone this operation, there not being any substance exposed to the air that could absorb any part of it.

I could not find any considerable alteration in the specific gravity of the air, in which candles, or brimstone, had burned out. I am satisfied, however, that it is not heavier than common air, which must have been manifest, if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose, I concluded that air, thus diminished in bulk, is

is rather lighter than common air, which favours the ſuppoſition of the fixed, or heavier part of the common air, having been precipitated.

An animal will live nearly, if not quite as long, in air in which candles have burned out, as in common air. This fact ſurprized me very greatly, having imagined that what is called the conſumption of air by flame, or reſpiration, to have been of the ſame nature; but I have ſince found, that this fact has been obſerved by many perſons, and even ſo early as by Mr. Boyle. I have alſo obſerved, that air in which brimſtone has burned, is not in the leaſt injurious to animals, after the fumes, which at firſt make it very cloudy, have intirely ſubſided.

Having read, in the Memoirs of the Society at Turin, Vol. I. p. 41. that air in which candles had burned out was perfectly reſtored, ſo that other candles would burn in it again as well as ever, after having been expoſed to a conſiderable degree of cold, and likewiſe after having been compreſſed in bladders (for the cold had been ſuppoſed to have produced this effect by nothing but condenſation): I repeated theſe experiments, and did, indeed, find, that, when I compreſſed the air in bladders, as the Count de Saluce, who made the obſervation, had done, the experiment ſucceeded: but having had ſufficient reaſon to diſtruſt bladders, I compreſſed the air in a glaſs veſſel ſtanding in water; and then I found, that this proceſs is altogether ineffectual for the purpoſe. I kept the air compreſſed much more, and much longer, than he had done, but without producing any alteration in it. I alſo find, that a greater degree of cold than that which he applied, and

of

of longer continuance, did by no means reſtore this kind of air: for when I have expoſed the phials which contained it a whole night, in which the froſt was very intenſe; and alſo when I kept it ſurrounded with a mixture of ſnow and ſalt, I found it, in all reſpects, the ſame as before.

It is alſo advanced, in the ſame Memoir, p. 41. that heat only, as the reverſe of cold, renders air unfit for candles burning in it. But I repeated the experiment of the Count for that purpoſe, without finding any ſuch effect from it. I alſo remember that, many years ago, I filled an exhauſted receiver with air, that had paſſed through a glaſs tube made red-hot, and found that a candle would burn in it perfectly well. Alſo, rarefaction by the air-pump does not injure air in the leaſt degree.

Though this experiment failed, I flatter myſelf that I have accidentally hit upon a method of reſtoring air which has been injured by the burning of candles, and that I have diſcovered at leaſt one of the reſtoratives which nature employs for this purpoſe. It is vegetation. In what manner this proceſs in nature operates, to produce ſo remarkable an effect, I do not pretend to have diſcovered; but a number of facts declare in favour of this hypotheſis. I ſhall introduce my account of them, by reciting ſome of the obſervations which I made on the growing of plants in confined air, which led to this diſcovery.

One might have imagined that, ſince common air is neceſſary to vegetable, as well as to animal life, both plants and animals had affected it in the ſame manner, and I own I had that expectation, when

when I firſt put a ſprig of mint into a glaſs-jar, ſtanding inverted in a veſſel of water; but when it had continued growing there for ſome months, I found that the air would neither extinguiſh a candle, nor was it at all inconvenient to a mouſe, which I put into it.

The plant was not affected any otherwiſe than was the neceſſary conſequence of its confined ſituation; for plants growing in ſeveral other kinds of air, were all affected in the very ſame manner. Every ſucceſſion of leaves was more diminiſhed in ſize than the preceding, till, at length, they came to be no bigger than the heads of pins. The root decayed, and the ſtalk alſo, beginning from the root; and yet the plant continued to grow upwards, drawing its nouriſhment through a black and rotten ſtem. In the third or fourth ſet of leaves, long hairy filaments grew from the inſertion of each leaf, and ſometimes from the body of the ſtem, ſhooting out as far as the veſſel in which it grew would permit, which, in my experiments, was about two inches. In this manner a ſprig of mint lived, the old ſtem decaying, and new ones ſhooting up in its place, but leſs and leſs continually, all the ſummer ſeaſon.

In repeating this experiment, care muſt be taken to draw away all the dead leaves from about the plant, leſt they ſhould putrefy, and affect the air. I have found that a freſh cabbage leaf, put under a glaſs veſſel filled with common air, for the ſpace of one night only, has ſo far affected the air, that a candle would not burn in it the next morning, and yet the leaf had not acquired any ſmell of putrefaction.

Finding

Finding that candles burn very well in air in which plants had grown a long time, and having had ſome reaſon to think, that there was ſomething attending vegetation, which reſtored air that had been injured by reſpiration, I thought it was poſſible that the ſame proceſs might alſo reſtore the air that had been injured by the burning of candles.

Accordingly, on the 17th of Auguſt, 1771, I put a ſprig of mint into a quantity of air, in which a wax candle had burned out, and found that, on the 27th of the ſame month, another candle burned perfectly well in it. This experiment I repeated, without the leaſt variation in the event, not leſs than eight or ten times in the remainder of the ſummer. Several times I divided the quantity of air in which the candle had burned out, into two parts, and putting the plant into one of them, left the other in the ſame expoſure, contained, alſo, in a glaſs veſſel immerſed in water, but without any plant; and never failed to find, that a candle would burn in the former, but not in the latter. I generally found that five or ſix days were ſufficient to reſtore this air, when the plant was in its vigour; whereas I have kept this kind of air in glaſs veſſels, immerſed in water many months, without being able to perceive that the leaſt alteration had been made in it. I have alſo tried a great variety of experiments upon it, as by condenſing, rarefying, expoſing to the light and heat, &c. and throwing into it the effluvia of many different ſubſtances, but without any effect.

Experiments made in the year 1772, abundantly confirmed my concluſion concerning the reſtoration of air, in which candles had burned out by plants growing

growing in it. The firſt of theſe experiments was made in the month of May; and they were frequently repeated in that and the two following months, without a ſingle failure.

For this purpoſe I uſed the flames of different ſubſtances, though I generally uſed wax or tallow candles. On the 24th of June the experiment ſucceeded perfectly well with air in which ſpirit of wine had burned out, and on the 27th of the ſame month it ſucceeded equally well with air in which brimſtone matches had burned out, an effect of which I had deſpaired the preceding year.

This reſtoration of air I found depended upon the vegetating ſtate of the plant; for though I kept a great number of the freſh leaves of mint in a ſmall quantity of air in which candles had burned out, and changed them frequently, for a long ſpace of time, I could perceive no melioration in the ſtate of the air.

This remarkable effect does not depend upon any thing peculiar to mint, which was the plant that I always made uſe of till July 1772; for on the 16th of that month, I found a quantity of this kind of air to be perfectly reſtored by ſprigs of balm, which had grown in it from the 7th of the ſame month.

That this reſtoration of air was not owing to any aromatic effluvia of theſe two plants, not only appeared by the eſſential oil of mint having no ſenſible effect of this kind; but from the equally complete reſtoration of this vitiated air by the plant called groundſel, which is uſually ranked among the weeds, and has an offenſive ſmell. This was the reſult of an experiment made the 16th of July, when the

 plant

plant had been growing in the burned air from the 8th of the ſame month. Beſides, the plant which I have found to be the moſt effectual of any that I have tried for this purpoſe is ſpinach, which is of quick growth, but will ſeldom thrive long in water. One jar of burned air was perfectly reſtored by this plant in four days, and another in two days. This laſt was obſerved on the 22d of July. In general this effect may be preſumed to have taken place in much leſs time than I have mentioned; becauſe I never choſe to make a trial of the air, till I was pretty ſure, from preceding obſervations, that the event which I had expected muſt have taken place, if it would ſucceed at all; leſt, returning back that part of the air on which I made the trial, and which would thereby neceſſarily receive a ſmall mixture of common air, the experiment might not be judged to be quite fair; though I myſelf might be ſufficiently ſatisfied with reſpect to the allowance that was to be made for that ſmall imperfection.

III.

Of inflammable Air.

I have generally made inflammable air in the manner deſcribed by Mr. Cavendiſh, in the Philoſophical Tranſactions, from iron, zinc, or tin; but chiefly from the two former metals, on account of the proceſs being the leaſt troubleſome: but when I extracted it from vegetable or animal ſubſtances, or from coals, I put them into a gun barrel, to the orifice of which I luted a glaſs tube, or the ſtem of a to-

a tobacco pipe, and to the end of this I tied a flaccid bladder, in order to catch the generated air.

There is not, I believe, any vegetable or animal ſubſtance whatever, nor any mineral ſubſtance, that is inflammable, but what will yield great plenty of inflammable air, when they are treated in this manner, and urged with a ſtrong heat; but, in order to get the moſt air, the heat muſt be applied as ſuddenly, and as vehemently, as poſſible. For, notwithſtanding the ſame care be taken in luting, and in every other reſpect, ſix or even ten times more air may be got by a ſudden heat than by a ſlow one, though the heat that is laſt applied be as intenſe as that which was applied ſuddenly. A bit of dry oak, weighing about twelve grains, will generally yield about a ſheep's bladder full of inflammable air with a briſk heat, when it will only give about two or three ounce meaſures if the ſame heat be applied to it very gradually. To what this difference is owing, I cannot tell.

Inflammable air, when it is made by a quick proceſs, has a very ſtrong and offenſive ſmell, from whatever ſubſtance it be generated; but this ſmell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal ſubſtances. The laſt is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, or from ſoft muſcular fleſh, or any other part of the animal. The burning of any ſubſtance occaſions the ſame ſmell: for the groſs fume which ariſes from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The ſmell of in-

flammable air is the very ſame, as far as I am able to perceive, from whatever ſubſtance of the ſame kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was obſerved before, from any part of an animal.

If a quantity of inflammable air be contained in a glaſs veſſel ſtanding in water, and have been generated very faſt, it will ſmell even through the water, and this water will alſo ſoon become covered with a thin film, aſſuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red okre, or the earth of iron, as I have found by collecting a conſiderable quantity of it; and if it have been generated from zinc, it is a whitiſh ſubſtance, which I ſuppoſe to be the calx of the metal. It likewiſe ſettles to the bottom of the veſſel, and when the water is ſtirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this ſcum for a conſiderable time after the air is removed from it. This I have often obſerved with reſpect to iron.

Inflammable air, made by a violent efferveſcence, I have obſerved to be much more inflammable than that which is made by a weak efferveſcence, whether the water or the oil of vitriol prevailed in the mixture. Alſo the offenſive ſmell was much ſtronger in the former caſe than in the latter. The greater degree of inflammability appeared by the greater number of ſucceſſive exploſions, when a candle was preſented to the neck of a phial filled with it. It is poſſible, however, that this diminution of inflammability

flammability may, in some measure, arise from the air continuing so much longer in the bladder when it is made very slowly; though I think the difference is too great for this cause to have produced the whole of it. It may, perhaps, deserve to be tried by a different process, without a bladder.

Inflammable air is not thought to be miscible with water, and when kept many months, seems, in general, to be as inflammable as ever. Indeed, when it is extracted from vegetable or animal substances, a part of it will be imbibed by the water in which it stands; but it may be presumed, that in this case, there was a mixture of fixed air extracted from the substance along with it. I have indisputable evidence, however, that inflammable air, standing long in water, has actually lost all its inflammability, and even come to extinguish flame much more than that air in which candles have burned out. After this change it appears to be greatly diminished in quantity, and it still continues to kill animals the moment they are put into it.

This very remarkable fact first occurred to my observation on the twenty-fifth of May 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately set by a common quart bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly mistaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was set by. That which had

been

been produced from zinc was not altered, and filled the bottle as at first.

Another instance of this kind occurred to my observation on the 19th of June 1772, when a quantity of air, half of which had been inflammable air from zinc, and half air in which mice had died, and which had been put together the 30th of July 1771, appeared not to be in the least inflammable, but extinguished flame, as much as any kind of air that I had ever tried. I think that, in all, I have had four instances of inflammable air losing its inflammability, while it stood in water.

Though air tainted with putrefaction extinguishes flame, I have not found that animals or vegetables putrefying in inflammable air render it less inflammable. But one quantity of inflammable air, which I had set by in May 1771, along with the others above mentioned, had had some putrid flesh in it; and this air had lost its inflammability, when it was examined at the same time with the other in the December following. The bottle in which this air had been kept, smelled exactly like very strong Harrowgate water. I do not think that any person could have distinguished them.

I have made plants grow for several months in inflammable air made from zinc, and also from oak; but, though the plants grew pretty well, the air still continued inflammable. The former, indeed, was not so highly inflammable as when it was fresh made, but the latter was quite as much so; and the diminution of inflammability in the former case, I attribute to some other cause than the growth of the plants.

No

No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of a spark is remarkably different in some different kinds of air, which seems to shew that they are not equally good non-conductors. In fixed air, the electric spark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, since the most vigorous sparks are always the whitest, and, in other cases, when the spark is red, there is reason to think that the electric matter passes with difficulty, and with less rapidity: it is possible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteness of the spark in the fixed air, may be owing to its meeting with no conducting particles at all. When an explosion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were still tinged with a beautiful purple. The degree of whiteness in this case was probably owing to the electric matter rushing with more violence in an explosion than in a common spark.

Inflammable air kills animals as suddenly as fixed air, and, as far as can be perceived, in the same manner, throwing them into convulsions, and thereby occasioning present death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become less noxious; but this did not appear to be the case; for I killed a great number of mice in a small quantity of this air, which I kept several months for this purpose, without its being at all sensibly mended; the last, as well as the first mouse, dying the moment it was put into it.

I once imagined that, ſince fixed and inflammable air are the reverſe of one another, in ſeveral remarkable properties, a mixture of them would make common air; and while I made the mixtures in bladders, I imagined that I had ſucceeded in my attempt; but I have ſince found that thin bladders do not ſufficiently prevent the air that is contained in them from mixing with the external air. Alſo corks will not ſufficiently confine different kinds of air, unleſs the phials in which they are confined be ſet with their mouths downwards, and a little water lie in the necks of them, which, indeed, is equivalent to the air ſtanding in veſſels immerſed in water. In this manner, however, I have kept different kinds of air for ſeveral years.

Whatever methods I took to promote the mixture of fixed and inflammable air, they were all ineffectual. I think it my duty, however, to recite the iſſue of an experiment or two of this kind, in which equal mixtures of theſe two kinds of air had ſtood near three years, as they ſeem to ſhew that they had in part affected one another, in that long ſpace of time. Theſe mixtures I examined April 27, 1771. One of them had ſtood in quickſilver, and the other in a corked phial, with a little water in it. On opening the latter in water, the water inſtantly ruſhed in, and filled almoſt half of the phial, and very little more was abſorbed afterwards. In this caſe the water in the phial had probably abſorbed a conſiderable part of the fixed air, ſo that the inflammable air was exceedingly rarefied; and yet the whole quantity that muſt have been rendered non-elaſtic was ten times more than the bulk of the water; and it has not

not been found that water can contain much more than its own bulk of fixed air. But in other cafes I have found the diminution of a quantity of air, and efpecially of fixed air, to be much greater than I could well account for by any kind of abforption.

The phial which had ftood immerfed in quickfilver had loft very little of its original quantity; and being now opened in water, and left there, along with a another phial, which was juft then filled, as this had been three years before, with air half inflammable and half fixed, I obferved that the quantity of both was diminifhed, by the abforption of the water, in the fame proportion.

Upon applying a candle to the mouths of the phials which had been kept three years, that which had ftood in quickfilver went off at one explofion, exactly as it would have done if there had been a mixture of common air, with the inflammable. As a good deal depends upon the apertures of the veffels in which the inflammable air is fixed, I mixed the two kinds of air in equal proportion in the fame phial, and after letting it ftand fome days in water, that the fixed air might be abforbed, I applied a candle to it; but it made ten or twelve explofions (ftopping the phial after each of them) before the inflammable matter was exhaufted.

The air which had been confined in the corked phial exploded in the very fame manner as an equal mixture of the two kinds of air in the fame phial, the experiment being made as foon as the fixed air was abforbed, as before; fo that, in this cafe, the two kinds of air did not feem to have affected one another at all.

 Con-

Confidering inflammable air as air united to or loaded with phlogifton, I expofed to it feveral fubftances, which are faid to have a near affinity with phlogifton, as oil of vitriol, and fpirit of nitre (the former for above a month), but without making any fenfible alteration in it.

I obferved, however, that inflammable air, mixed with the fumes of fmoaking fpirit of nitre, goes off at one explofion, exactly like a mixture of half common and half inflammable air. This I tried feveral times, by throwing the inflammable air into a phial full of fpirit of nitre, with its mouth immerfed in a bafon containing fome of the fame fpirit, and then applying the flame of a candle to the mouth of the phial, the moment that it was uncovered, after it had been taken out of the bafon. This remarkable effect I haftily concluded to have arifen from the inflammable air having been in part deprived of its inflammability, by means of the ftronger affinity, which the fpirit of nitre had with phlogifton, and therefore I imagined that by letting them ftand longer in contact, and efpecially by agitating them ftrongly together, I fhould deprive the air of all its inflammability; but neither of thefe operations fucceeded, for ftill the air was only exploded at once, as before. And laftly, when I paffed a quantity of inflammable air, which had been mixed with the fumes of fpirit of nitre, through a body of water, and received it in another veffel, it appeared not to have undergone any change at all, for it went off in feveral fucceffive explofions, like the pureft inflammable air. The effect abovementioned muft, therefore, have been owing to the fumes of the fpirit of nitre fupplying

the

the place of common air for the purpoſe of ignition, which is analogous to other experiments with nitre.

Having had the curioſity, on the 25th of July 1772, to expoſe a great variety of different kinds of air to water out of which the air it contained had been boiled, without any particular view; the reſult was, in ſeveral reſpects, altogether unexpected, and led to a variety of new obſervations on the properties and affinities of ſeveral kinds of air with reſpect to water. Among the reſt three fourths of that which was inflammable was abſorbed by the water in about two days, and the remainder was inflammable, but weakly ſo.

Upon this, I began to agitate a quantity of ſtrong inflammable air in a glaſs jar, ſtanding in a pretty large trough of water, the ſurface of which was expoſed to the common air, and I found that when I had continued the operation about ten minutes, near one fourth of the quantity of air had diſappeared; and finding that the remainder made an effervefcence with nitrous air, I concluded that it muſt have become fit for reſpiration, whereas this kind of air is, at the firſt, as noxious as any other kind whatever. To aſcertain this, I put a mouſe into a veſſel containing 2½ ounce meaſures of it, and obſerved that it lived in it twenty minutes, which is as long as a mouſe will generally live in the ſame quantity of common air. This mouſe was even taken out alive, and recovered very well. Still alſo the air in which it had breathed ſo long was inflammable, though very weakly ſo. I have even found it to be ſo when a mouſe has actually died in it.

Inflammable air thus diminiſhed by agitation in water, makes but one exploſion on the approach of a candle exactly like a mixture of inflammable air with common air.

From this experiment I concluded that, by continuing the ſame proceſs, I ſhould deprive inflammable air of all its inflammability, and this I found to be the caſe; for, after a longer agitation, it admitted a candle to burn in it, like common air, only more faintly; and indeed by the teſt of nitrous air it did not appear to be near ſo good as common air. Continuing the ſame proceſs ſtill farther, the air which had been moſt ſtrongly inflammable a little before, came to extinguiſh a candle, exactly like air in which a candle had burned out, nor could they be diſtinguiſhed by the teſt of nitrous air.

I found, by repeated trials, that it was difficult to catch the time in which inflammable air obtained from metals, in coming to extinguiſh flame, was in the ſtate of common air, ſo that the tranſition from the one to the other muſt be very ſhort. I readily, however, found this ſtate in a quantity of inflammable air extracted from oak, which air I had kept by me a year, and in which a plant had grown, though very poorly, for ſome part of the time. A quantity of this air, after being agitated in water till it was diminiſhed about one half, admitted a candle to burn in it exceedingly well, and was even hardly to be diſtinguiſhed from common air by the teſt of nitrous air.

I took ſome pains to aſcertain the quantity of diminution, in freſh made and very highly inflammable air from iron, at which it ceaſed to be inflammable,

mable, and, upon the whole, I concluded that it was so when it was diminiſhed a little more than one half: for a quantity which was diminiſhed exactly one half had ſomething inflammable in it, but in the ſlighteſt degree imaginable.

Finding that water would imbibe inflammable air, I endeavoured to impregnate water with it, by the ſame proceſs by which I had made water imbibe fixed air; but though I found that diſtilled water would imbibe about one fourteenth of its bulk of inflammable air, I could not perceive that the taſte of it was ſenſibly altered.

IV.

Of Air infected with animal respiration, or putrefaction.

That candles will burn only a certain time, is a fact not better known, than it is that animals can live only a certain time, in a given quantity of air; but the cauſe of the death of the animal is not better known than that of the extinction of flame in the ſame circumſtances; and when once any quantity of air has been rendered noxious by animals breathing in it as long as they could, I do not know that any methods have been diſcovered of rendering it fit for breathing again. It is evident, however, that there muſt be ſome proviſion in nature for this purpoſe, as well as for that of rendering the air fit for ſuſtaining flame; for without it the whole maſs of the atmoſphere would, in time, become unfit for the purpoſe of animal life; and yet there is no reaſon to think that it is, at preſent, at all leſs fit for reſpiration than

it

it has ever been. I flatter myſelf, however, that I have hit upon two of the methods employed by nature for this great purpoſe. How many others there may be, I cannot tell.

When animals die upon being put into air in which other animals have died, after breathing in it as long as they could, it is plain that the cauſe of their death is not the want of any *pabulum vitæ*, which has been ſuppoſed to be contained in the air, but on account of the air being impregnated with ſomething ſtimulating to their lungs; for they almoſt always die in convulſions, and are ſometimes affected ſo ſuddenly, that they are irrecoverable after a ſingle inſpiration, though they be withdrawn immediately, and every method has been taken to bring them to life again. They are affected in the ſame manner, when they are killed in any other kind of noxious air that I have tried, viz. fixed air, inflammable air, air filled with the fumes of brimſtone, infected with putrid matter, in which a mixture of iron filings and brimſtone has ſtood, or in which charcoal has been burned, or metals calcined, or in nitrous air, &c.

If a mouſe (which is an animal that I have commonly made uſe of for the purpoſe of theſe experiments) can ſtand the firſt ſhock of this ſtimulus, or has been habituated to it by degrees, it will live a conſiderable time in air in which other mice will die inſtantaneouſly. I have frequently found that when a number of mice have been confined in a given quantity of air, leſs than half the time that they have actually lived in it, a freſh mouſe has been inſtantly thrown into convulſions, and died upon being put to them. It is evident, therefore, that if the

the experiment of the Black Hole were to be repeated, a man would ſtand the better chance of ſurviving it, who ſhould enter at the firſt, than at the laſt hour. I have alſo obſerved, that young mice will always live much longer than old ones, or than thoſe which are full grown, when they are confined in the ſame quantity of air. I have ſometimes known a young mouſe to live ſix hours in the ſame circumſtances in which an old mouſe has not lived one. On theſe accounts, experiments with mice, and, for the ſame reaſon, no doubt, with other animals alſo, have a conſiderable degree of uncertainty attending them; and therefore, it is neceſſary to repeat them frequently, before the reſult can be abſolutely depended upon.

The diſcovery of the proviſion in nature for reſtoring air, which has been injured by the reſpiration of animals, having long appeared to me to be one of the moſt important problems in natural philoſophy, I have tried a great variety of ſchemes in order to effect it. In theſe, my guide has generally been to conſider the influences to which the atmoſphere is, in fact, expoſed; and, as ſome of my unſucceſsful trials may be of uſe to thoſe who are diſpoſed to take pains in the farther inveſtigation of this ſubject, I ſhall mention the principal of them.

The noxious effluvium with which air is loaded by animal reſpiration, is not abſorbed by ſtanding without agitation in freſh or ſalt water. I have kept it many months in freſh water, when, inſtead of being meliorated, it has ſeemed to become even more deadly, ſo as to require more time to reſtore it, by the methods which will be explained hereafter, than

air

air which has been lately made noxious. I have even ſpent ſeveral hours in pouring this air from one glaſs veſſel into another, in water, ſometimes as cold, and ſometimes as warm, as my hands could bear it, and have ſometimes alſo wiped the veſſels many times, during the courſe of the experiment, in order to take off that part of the noxious matter, which might adhere to the glaſs veſſels, and which evidently gave them an offenſive ſmell; but all theſe methods were generally without any ſenſible effect. The motion, alſo, which the air received in theſe circumſtances, it is very evident, was of no uſe for this purpoſe.

This kind of air is not reſtored by being expoſed to the light, or by any other influence to which it is expoſed, when confined in a thin phial, in the open air, for ſome months.

Among other experiments, I tried a great variety of different effluvia, which are continually exhaling into the air, eſpecially of thoſe ſubſtances which are known to reſiſt putrefaction; but I could not by theſe means effect any melioration of the noxious quality of this kind of air.

Having read, in the Memoirs of the Imperial Society, of a plague not afflicting a particular village, in which there was a large ſulphur work, I immediately fumigated a quantity of this kind of air; or (which will hereafter appear to be the very ſame thing) air tainted with putrefaction, with the fumes of burning brimſtone, but without any effect.

I once imagined, that the nitrous acid in the air might be the general reſtorative which I was in queſt of; and the conjecture was favoured, by finding

ing that candles would burn, and animals live, in air extracted from ſaltpetre. I therefore ſpent a good deal of time in attempting, by a burning-glaſs, and other means, to impregnate this noxious air with ſome effluvium of ſaltpetre, and, with the ſame view, introduced into it the fumes of the ſmoaking ſpirit of nitre; but both theſe methods were altogether ineffectual.

In order to try the effect of heat, I put a quantity of air, in which mice had died, into a bladder, tied to the end of the ſtem of a tobacco-pipe, at the other end of which was another bladder, out of which the air was carefully preſſed. I then put the middle part of the ſtem into a chafing-diſh of hot coals, ſtrongly urged with a pair of bellows; and, preſſing the bladders alternately, I made the air paſs ſeveral times through the heated part of the pipe. I have alſo made this kind of air very hot, ſtanding in water before the fire. But neither of theſe methods were of any uſe.

Rarefaction and condenſation by inſtruments were alſo tried, but in vain.

Thinking it poſſible that the earth might imbibe the noxious quality of the air, and thence ſupply the roots of plants with ſuch putreſcent matter as is known to be nutritive to them, I kept a quantity of air, in which mice had died, in a phial, one half of which was filled with fine garden mould; but, though it ſtood two months in theſe circumſtances, it was not the better for it.

I once imagined that, ſince ſeveral kinds of air cannot be long ſeparated from common air, by being confined in bladders, in bottles well corked, or even

 cloſed

cloſed with ground ſtopples, the affinity between this noxious air and the common air might be ſo great, that they would mix through a body of water interpoſed between them; the water continually receiving from the one, and giving to the other, eſpecially as water receives ſome kinds of impregnation from, I believe, every kind of air to which it is contiguous; but I have ſeen no reaſon to conclude, that a mixture of any kind of air with the common air can be produced in this manner. I have kept air in which mice have died, air in which candles have burned out, and inflammable air, ſeparated from the common air, by the ſlighteſt partition of water that I could well make, ſo that it might not evaporate in a day or two, if I ſhould happen not to attend to them; but I found no change in them after a month or ſix weeks. The inflammable air was ſtill inflammable, mice died inſtantly in the air in which other mice had died before, and candles would not burn where they had burned out before.

Since air tainted with animal or vegetable putrefaction is the ſame thing with air rendered noxious by animal reſpiration, I ſhall now recite the obſervations which I have made upon this kind of air, before I treat of the method of reſtoring them.

That theſe two kinds of air are, in fact, the ſame thing, I conclude from their having ſeveral remarkable common properties, and from their differing in nothing that I have been able to obſerve. They equally extinguiſh flame, they are equally noxious to animals, they are equally, and in the ſame way, offenſive to the ſmell, they are equally diminiſhed

in their quantity, they equally precipitate in lime-water, and they are reſtored by the ſame means.

Since air which has paſſed through the lungs is the ſame thing with air tainted with animal putrefaction, it is probable that one uſe of the lungs is to carry off a putrid effluvium, without which, perhaps, a living body might putrefy as ſoon as a dead one.

When a mouſe putrefies in any given quantity of air, the bulk of it is generally increaſed for a few days; but in a few days more it begins to ſhrink up, and generally, in about eight or ten days, if the weather be pretty warm, it will be found to be diminiſhed $\frac{1}{6}$, or $\frac{1}{5}$ of its bulk. If it do not appear to be diminiſhed after this time, it only requires to be paſſed through water, and the diminution will not fail to be ſenſible. I have ſometimes known almoſt the whole diminution to take place, upon once or twice paſſing through the water. The ſame is the caſe with air, in which animals have breathed as long as they could. Alſo, air in which candles have burned out may almoſt always be farther reduced by this means. All theſe proceſſes, as I obſerved before, ſeem to diſpoſe the compound maſs of air to part with ſome conſtituent part belonging to it; and this being miſcible with water, muſt be brought into contact with it, in order to mix with it to the moſt advantage, eſpecially when its union with the other conſtituent principles of the air is but partially broken.

I have put mice into veſſels which had their mouths immerſed in quickſilver, and obſerved that the air was not much contracted after they were dead or cold; but upon withdrawing the mice, and admitting

lime-water to the air it immediately became turbid, and was contracted in its dimensions as usual.

I tried the same thing with air tainted with putrefaction, putting a dead mouse to a quantity of common air, in a vessel which had its mouth immersed in quicksilver, and after a week I took the mouse out, drawing it through the quicksilver, and observed that for some time there was an apparent increase of the air perhaps about $\frac{1}{20}$. After this, it stood two days in the quicksilver, without any sensible alteration; and then admitting water to it, it began to be absorbed, and continued so, till the original quantity was diminished about $\frac{1}{6}$. If, instead of common water, I had made use of lime water in this experiment, I make no doubt but it would have become turbid.

If a quantity of lime-water in a phial be put under a glass vessel standing in water, it will not become turbid, and provided the access of the common air be prevented, it will continue lime-water, I do not know how long; but if a mouse be left to putrefy in the vessel, the water will deposit all its lime in a few days. This may be owing to the fixed air being transferred from the putrid mouse into the water, and yet it is evident that there is a putrid effluvium intirely distinct from this kind of air, and which has very different properties.

It is a doubt with me, however, whether the putrid effluvium be not chiefly fixed air, with the addition of some other effluvium, which has the power of diminishing common air. The resemblance between the true putrid effluvium and fixed air in the following experiment, which is as decisive

as

as I can poſſibly contrive it, appeared to be very great; indeed, much greater than I had expected. I put a dead mouſe into a tall glaſs veſſel, and having filled the remainder with quickſilver, and ſet it, inverted, in a pot of quickſilver, I let it ſtand about two months, in which time the putrid effluvium iſſuing from the mouſe had filled the whole veſſel, and part of the diſſolved blood, which lodged upon the ſurface of the quikſilver, began to be thrown out. I then filled another glaſs veſſel, of the ſame ſize and ſhape, with as pure fixed air as I could make, and expoſed them both, at the ſame time, to a quantity of lime-water. In both caſes the water grew turbid alike, it roſe equally faſt in both the veſſels, and likewiſe equally high; ſo that about the ſame quantity remained unabſorbed by the water. One of theſe kinds of air, however, was exceedingly ſweet and pleaſant, and the other inſufferably offenſive; one of them alſo would have made an addition to any quantity of common air with which it had been mixed, and the other would have diminiſhed it. This, at leaſt, would have been the conſequence, if the mouſe itſelf had putrefied in any quantity of air.

It ſeems to depend, in ſome meaſure, upon the time, and other circumſtances, in the diſſolution of animal or vegetable ſubſtances, whether they yield the proper putrid effluvium, or fixed, or inflammable air; but the experiments which I have made upon this ſubject, have not been numerous enough to enable me to decide with certainty concerning thoſe circumſtances. Putrid cabbage, green, or boiled, infects the air in the very ſame manner as putrid animal ſubſtances. Air thus tainted is equally contracted

in

in its dimensions, it equally extinguishes flame, and is equally noxious to animals; but they affect the air very differently if the heat that is applied to them be considerable. If beef or mutton, raw, or boiled, be placed so near to the fire, that the heat to which it is exposed shall equal, or rather exceed, that of the blood, a considerable quantity of air will be generated in a day or two, about ⅟th of which I have generally found to be absorbed by water, while all the rest was inflammable; but air generated from vegetables, in the same circumstances, will be almost all fixed, and no part of it inflammable. This I have repeated again and again, the whole process being in quicksilver; so that neither common air, nor water, had any access to the substance on which the experiment was made; and the generation of air, or effluvium of any kind, except what might be absorbed by quicksilver, or resorbed by the substance itself, might be distinctly noted.

A vegetable substance, after standing a day or two in these circumstances, will yield nearly all the air that can be extracted from it, in that degree of heat; whereas an animal substance will continue to give more air or effluvium, of some kind or other, with very little alteration, for many weeks. It is remarkable, however, that though a piece of beef or mutton, plunged in quicksilver, and kept in this degree of heat, yield air, the bulk of which is inflammable, and contracts no putrid smell (at least, in a day or two), a mouse treated in the same manner, yields the proper putrid effluvium, as, indeed the smell sufficiently indicates; and this effluvium does

either

either itſelf extinguiſh flame, or has in it ſuch a mixture of fixed air, as to give it that property.

That the putrid effluvium will mix with water ſeems to be evident from the following experiment. If a mouſe be put into a jar full of water, ſtanding with its mouth inverted in another veſſel of water, a conſiderable quantity of elaſtic matter (and which may, therefore, be called air) will ſoon be generated, unleſs the weather be ſo cold as to check all putrefaction. After a ſhort time, the water contracts an extremely fetid and offenſive ſmell, which ſeems to indicate that the putrid effluvium pervades the water, and affects the neighbouring air; and ſince, after this, there is often no increaſe of the air, that ſeems to be the very ſubſtance which is carried off through the water, as faſt as it is generated; and the offenſive ſmell is a ſufficient proof that it is not fixed air. For this has a very agreeable flavour, whether it be produced by fermentation, or extracted from chalk by oil of vitriol; affecting not only the mouth, but even the noſtrils, with a pungency which is peculiarly pleaſing to a certain degree, as any perſon may eaſily ſatisfy himſelf who will chuſe to make the experiment. If the water in which the mouſe was immerſed, and which is ſaturated with the putrid air, be changed, the greater part of the putrid air will, in a day or two, be abſorbed, though the mouſe continues to yield the putrid effluvium as before; for as ſoon as this freſh water becomes ſaturated with it, it begins to be offenſive to the ſmell, and the quantity of the putrid air upon its ſurface increaſes as before. I kept a mouſe producing putrid air in this manner for the ſpace of ſeveral months.

Six

Six ounce meaſures of air not readily abſorbed by water, appeared to have been generated from one mouſe, which had been putrefying eleven days in confined air, before it was put into a jar which was quite filled with water, for the purpoſe of this obſervation.

Air thus generated from putrid mice ſtanding in water, without any mixture of common air, extinguiſhes flame, and is noxious to animals, but not more ſo than common air only tainted with putrefaction. It is exceedingly difficult and tedious to collect a quantity of this putrid air, not miſcible in water, ſo very great a proportion of what is collected being abſorbed by the water, in which it is kept; but what that proportion is, I have not endeavoured to aſcertain.

Though a quantity of air be diminiſhed by any ſubſtance putrefying in it, I have not yet found the ſame effect to be produced by a mixture of putrid air with common air; but, in the manner in which I have hitherto made the experiment, I was obliged to let the putrid air, paſs through a body of water; which might inſtantly abſorb whatever it was in the putrid ſubſtance, that diminiſhed the common air.

Inſects of various kinds live perfectly well in air tainted with animal or vegetable putrefaction, when a ſingle inſpiration of it would have inſtantly killed any animal. I have frequently tried the experiment with flies and butterflies. I have alſo obſerved, that the *aphides* will thrive as well upon plants growing in this kind of air, as in the open air. I have even been frequently obliged to take plants out of the putrid air in which they were growing, on purpoſe to bruſh away the ſwarms of

these insects which infected them; and yet so effectually did some of them conceal themselves, and so fast did they multiply, in these circumstances, that I could seldom keep the plants quite clear of them.

When air has been freshly and strongly tainted with putrefaction, so as to smell through the water, sprigs of mint have presently died, upon being put into it, their leaves turning black; but if they do not die presently, they thrive in a most surprizing manner. In no other circumstances have I ever seen vegetation so vigorous as in this kind of air, which is immediately fatal to animal life. Though these plants have been crouded in jars filled with this air, every leaf has been full of life; fresh shoots have branched out in various directions, and have grown much faster than other similar plants, growing in the same exposure in common air.

This observation led me to conclude, that plants, instead of affecting the air in the same manner with animal respiration, reverse the effects of breathing, and tend to keep the atmosphere sweet and wholesome, when it is become noxious, in consequence of animals living and breathing, or dying and putrefying in it.

In order to ascertain this, I took a quantity of air, made thoroughly noxious, by mice breathing and dying in it, and divided it into two parts; one of which I put into a phial immersed in water; and to the other (which was contained in a glass jar, standing in water) I put a sprig of mint. This was about the beginning of August 1771, and after eight or nine days, I found that a mouse lived perfectly well

 in

in that part of the air, in which the ſprig of mint had grown, but died the moment it was put into the other part of the ſame original quantity of air; and which I had kept in the very ſame expoſure, but without any plant growing in it.

This experiment I have ſeveral times repeated; ſometimes uſing air, in which animals had breathed and died; ſometimes uſing air tainted with vegetable or animal putrefaction, and generally with the ſame ſucceſs.

Once, I let a mouſe live and die in a quantity of air, which had been noxious, but which had been reſtored by this proceſs, and it lived nearly as long as I conjectured it might have done in an equal quantity of freſh air; but, this is ſo exceedingly various, that it is not eaſy to form any judgment from it; and in this caſe the ſymptom of *difficult reſpiration* ſeemed to begin earlier than it would have done in common air.

Since the plants that I made uſe of manifeſtly grow and thrive in putrid air; ſince putrid matter is well known to afford proper nouriſhment for the roots of plants; and ſince it is likewiſe certain that they receive nouriſhment by their leaves as well as by their roots, it ſeems to be exceedingly probable, that the putrid effluvium is in ſome meaſure extracted from the air, by means of the leaves of plants, and therefore that they render the remainder more fit for reſpiration.

Towards the end of the year ſome experiments of this kind did not anſwer ſo well as they had done before, and I had inſtances of the relapſing of this reſtored air to its former noxious ſtate. I therefore
ſuſpended

ſuſpended my judgment concerning the efficacy of plants to reſtore this kind of noxious air, till I ſhould have an opportunity of repeating my experiments, and giving more attention to them. Accordingly I reſumed the experiments in the ſummer of the year 1772, when I preſently had the moſt indiſputable proof of the reſtoration of putrid air by vegetation; and as the fact is of ſome importance, and the ſubſequent variation in the ſtate of this kind of air is a little remarkable; I think it neceſſary to relate ſome of the facts pretty circumſtantially.

The air, on which I made the firſt experiments, was rendered exceedingly noxious by mice dying in it on the 20th of June. Into a jar nearly filled with one part of this air, I put a ſprig of mint, while I kept another part of it in a phial, in the ſame expoſure; and on the 27th of the ſame month, and not before, I made a trial of it, by introducing a mouſe into a glaſs veſſel, containing $2\frac{1}{2}$ ounce meaſures filled with each kind of air; and I noted the following facts.

When the veſſel was filled with the air in which the mint had grown, a very large mouſe lived five minutes in it, before it began to ſhew any ſign of uneaſineſs. I then took it out, and found it to be as ſtrong and vigorous as when it was firſt put in; whereas in that air which had been kept in the phial only, without a plant growing in it, a younger mouſe continued not longer than two or three ſeconds, and was taken out quite dead. It never breathed after, and was immediately motionleſs. After half an hour, in which time the larger mouſe

 (which

(which I had kept alive, that the experiment might be made on both the kinds of air with the very same animal) would have been sufficiently recruited, supposing it to have received any injury by the former experiment, was put into the same vessel of air; but though it was withdrawn again, after being in it hardly one second, it was recovered with difficulty, not being able to stir from the place for near a minute. After two days, I put the same mouse into an equal quantity of common air, and observed that it continued seven minutes without any sign of uneasiness; and being very uneasy after three minutes longer, I took it out. Upon the whole, I concluded that the restored air wanted about one fourth of being as wholesome as common air. The same thing also appeared when I applied the test of nitrous air.

In the seven days, in which the mint was growing in this jar of noxious air, three old shoots had extended themselves about three inches, and several new ones had made their appearance in the same time. Dr. Franklin and Sir John Pringle happened to be with me, when the plant had been three or four days in this state, and took notice of its vigorous vegetation, and remarkably healthy appearance in that confinement.

On the 30th of the same month, a mouse lived fourteen minutes, breathing naturally all the time, and without appearing to be much uneasy, till the last two minutes, in air which had been rendered noxious by mice breathing in it almost a year before, and which I had found to be most highly noxious on the 19th of this month, a plant having grown in it, but

but not exceedingly well, these eleven days; on which account, I had deferred making the trial so long. This restored air was affected by a mixture of nitrous air, almost as much as common air.

As this putrid air was thus easily restored to a considerable degree of fitness for respiration, by plants growing in it, I was in hopes that by the same means it might in time be so much more perfectly restored, that a candle would burn in it; and for this purpose I kept plants growing in the jars which contained this air till the middle of August following, but did not take sufficient care to pull out all the old and rotten leaves. The plants, however, had grown, and looked so well upon the whole, that I had no doubt but that the air must constantly have been in a mending state; when I was exceedingly surprized to find, on the 24th of that month, that though the air in one of the jars had not grown worse, it was no better, and that the air in the other jar was so much worse than it had been, that a mouse would have died in it in a few seconds. It also made no effervescence with nitrous air, as it had done before.

Suspecting that the same plant might be capable of restoring putrid air to a certain degree only, or that plants might have a contrary tendency in some stages of their growth, I withdrew the old plant, and put a fresh one in its place; and found that, after seven days, the air was restored to its former wholesome state. This fact I consider as a very remarkable one, and well deserving of a farther investigation, as it may throw more light upon the principles of vegetation. It is not, however,

 a single

a single fact; for I had several instances of the same kind in the preceding year; but it seemed so very extraordinary, that air should grow worse by the continuance of the same treatment by which it had grown better, that, whenever I observed it, I concluded that I had not taken sufficient care to satisfy myself of its previous restoration.

That plants are capable of perfectly restoring air injured by respiration, may, I think, be inferred with certainty from the perfect restoration, by this means, of air which had passed through my lungs, so that a candle would burn in it again, though it had extinguished flame before, and a part of the same original quantity of air still continued to do so. Of this one instance occurred in the year 1771, a sprig of mint having grown in a jar of this kind of air, from the 25th of July to the 17th of August following; and another trial I made with the same success the 7th of July 1772, the plant having grown in it from the 29th of June preceding. In this case also I found that the effect was not owing to any virtue in the leaves of mint; for I kept them constantly changed in a quantity of this kind of air, for a considerable time, without making any sensible alteration in it.

These proofs of a partial restoration of air by plants in a state of vegetation, though in a confined and unnatural situation, cannot but render it highly probable, that the injury which is continually done to the atmosphere by the respiration of such a number of animals, and the putrefaction of such masses of both vegetable and animal matter, is, in part at least, repaired by the vegetable creation.

And,

And, notwithſtanding the prodigious maſs of air that is corrupted daily by the abovementioned cauſes; yet, if we conſider the immenſe profuſion of vegetables upon the face of the earth, growing in places ſuited to their nature, and conſequently at full liberty to exert all their powers, both inhaling and exhaling, it can hardly be thought, but that it may be a ſufficient counterbalance to it, and that the remedy is adequate to the evil.

Dr. Franklin, who, as I have already obſerved, ſaw ſome of my plants in a very flouriſhing ſtate, in highly noxious air, was pleaſed to expreſs very great ſatisfaction with the reſult of the experiments. In his anſwer to the letter in which I informed him of it, he ſays,

" That the vegetable creation ſhould reſtore the
" air which is ſpoiled by the animal part of it,
" looks like a rational ſyſtem, and ſeems to be of
" a piece with the reſt. Thus fire purifies water
" all the world over. It purifies it by diſtillation,
" when it raiſes it in vapours, and lets it fall in
" rain; and farther ſtill by filtration, when, keep-
" ing it fluid, it ſuffers that rain to percolate the
" earth. We knew before, that putrid animal ſub-
" ſtances were converted into ſweet vegetables,
" when mixed with the earth, and applied as
" manure; and now, it ſeems, that the ſame pu-
" trid ſubſtances, mixed with the air, have a ſimi-
" lar effect. The ſtrong thriving ſtate of your
" mint in putrid air ſeems to ſhew that the air is
" mended by taking ſomething from it, and not
" by adding to it." He adds, " I hope this will
" give ſome check to the rage of deſtroying trees

"that grow near houses, which has accompanied "our late improvements in gardening, from an "opinion of their being unwholesome. I am certain, from long observation, that there is nothing unhealthy in the air of woods; for we "Americans have every where our country habitations in the midst of woods, and no people on "earth enjoy better health, or are more prolific."

Having rendered inflammable air perfectly innoxious by continued agitation in a trough of water, deprived of its air, I concluded that other kinds of noxious air might be restored by the same means; and I presently found that this was the case with putrid air, even of more than a year's standing. I shall observe once for all, that this process has never failed to restore any kind of noxious air on which I have tried it, viz. air injured by respiration or putrefaction, air infected with the fumes of burning charcoal, and of calcined metals, air in which a mixture of iron filings and brimstone, or that in which paint made of white lead and oil has stood, or air which has been diminished by a mixture of nitrous air. Of the remarkable effect which this process has on nitrous air itself, an account will be given in its proper place.

If this process be made in water deprived of air, either by the air pump, by boiling, by distillation, or if fresh rain water be used, the air will always be diminished by the agitation; and this is certainly the fairest method of making the experiment. If the water be fresh pump water, there will always be an increase of the air by agitation, the air contained in the water being set loose, and joining

joining that which is in the jar. In this caſe, alſo, the air has never failed to be reſtored; but then it might be ſuſpected that the melioration was produced by the addition of ſome more wholeſome ingredient. As theſe agitations were made in jars with wide mouths, and in a trough which had a large ſurface expoſed to the common air, I take it for granted that the noxious effluvia, whatever they be, were firſt imbibed by the water, and thereby tranſmitted to the common atmoſphere. In ſome caſes this was ſufficiently indicated by the diſagreeable ſmell which attended the operation.

After I had made theſe experiments, I was informed that an ingenious phyſician and philoſopher had kept a fowl alive twenty-four hour, in a quantity of air in which another fowl of the ſame ſize had not been able to live longer than an hour, by contriving to make the air, which it breathed, paſs through no very large quantity of acidulated water, the ſurface of which was not expoſed to the common air; and that even when the water was not acidulated, the fowl lived much longer than it could have done, if the air which it breathed had not been drawn through the water. As I ſhould not have concluded that this experiment would have ſucceeded ſo well, from any obſervations that I had made upon the ſubject, I took a quantity of air in which mice had died, and agitated it very ſtrongly, firſt in about five times its own quantity of diſtilled water, in the manner in which I had impregnated water with fixed air; but though the operation was continued a long time, it made no ſenſible change in the properties of the air. I alſo repeated the operation with

 pump

pump water, but with as little effect. In this case, however, though the air was agitated in a phial, which had a narrow neck, the furface of the water in the bafon was confiderably large, and expofed to the common atmofphere, which muft have tended a little to favour the experiment. In order to judge more precifely of the effect of thefe different methods of agitating air, I transfered the very noxious air, which I had not been able to amend in the leaft degree by the former method, into an open jar, ftanding in a trough of water; and when I had agitated it till it was diminifhed about one third, I found it to be better than air, in which candles had burned out, as appeared by the teft of the nitrous air; and a moufe lived in 2 ½ ounce meafures of it a quarter of an hour, and was not fenfibly affected the firft ten or twelve minutes.

In order to determine whether the addition of any *acid* to the water, would make it more capable of reftoring putrid air, I agitated a quantity of it in a phial containing very ftrong vinegar; and after that in *aqua fortis*, only half diluted with water; but, by neither of thefe proceffes was the air at all mended, though the agitation was repeated at intervals during a whole day, and it was moreover allowed to ftand in that fituation all night.

Since, however, water in thofe experiments muft have imbibed and retained a certain portion of the noxious effluvia, before they could be tranfmited to the external air, I do not think it improbable but that the agitation of the fea and large lakes may be of fome ufe for the purification of the atmofphere, and the putrid matter contained in water may be

imbibed

imbibed by aquatic plants, or be depoſited in ſome other manner.

Having found, by ſeveral experiments abovementioned, that the proper putrid effluvium is ſomething quite diſtinct from fixed air, and finding, by the experiments of Dr. Macbride, that fixed air corrects putrefaction; I once concluded that this effect was produced, not by ſtopping the flight of the fixed air, or reſtoring to the putrefying ſubſtance the very ſame thing that had eſcaped from it; and which was the common vinculum of all its parts (which is that ingenious author's hypotheſis) but by an affinity between the fixed air and the putrid effluvium. It therefore occurred to me, that fixed air, and air tainted with putrefaction, though equally noxious when ſeparate, might make a wholeſome mixture, the one correcting the other; and I was confirmed in this opinion by, I believe, not leſs than fifty or ſixty inſtances, in which air, that had been made in the higheſt degree noxious, by reſpiration or putrefaction, was ſo far ſweetened, by a mixture of about four times as much fixed air that afterwards mice lived in it exceedingly well, and in ſome caſes almoſt as long as in common air. I found it, indeed, to be more difficult to reſtore old putrid air by this means; but I hardly ever failed to do it, when the two kinds of air had ſtood a long time together, by which I mean about a fortnight or three weeks.

The reaſon why I do not abſolutely conclude that the reſtoration of air in theſe caſes was the effect of fixed air, is that, when I made a trial of the mixture, I ſometimes agitated the two kinds

 of

of air pretty ſtrongly together, in a trough of water, or at leaſt paſſed it ſeveral times through the water, from one jar to another, that the ſuperfluous fixed air might be abſorbed, not ſuſpecting at that time that the agitation could have any other effect; but having ſince found that very violent, and eſpecially long continued agitation in water, without any mixture of fixed air, never failed to render any kind of noxious air in ſome meaſure fit for reſpiration (and in one particular inſtance the mere transferring of the air from one veſſel to another through the water, though for a much longer time than I ever uſed for the mixtures of air, was of conſiderable uſe for the ſame purpoſe); I began to entertain ſome doubt of the efficacy of fixed air, for that purpoſe. In ſome caſes alſo the mixture of fixed air had by no means ſo much effect on the putrid air as, from the generality of my obſervations, I ſhould have expected.

I was always aware, indeed, that it might be ſaid, that, the reſiduum of fixed air not being very noxious, ſuch an addition muſt contribute to mend the putrid air; but, in order to obviate this objection, I once mixed the reſiduum of as much fixed air as I had found, by a variety of trials, to be ſufficient to reſtore a given quantity of putrid air, with an equal quantity of putrid air, without making any ſenſible melioration of it.

Upon the whole, I am inclined to think that this proceſs could hardly have ſucceeded ſo well as it did with me, and in ſo great a number of trials, unleſs fixed air have ſome tendency to correct air tainted with reſpiration or putrefaction; and it is

5 perfectly

perfectly agreeable to the analogy of Dr. Macbride's discoveries, and may naturally be expected from them, that it should have such an effect.

By a mixture of fixed air I have made wholesome the residuum of air generated by putrefaction only, from mice plunged in water. This, one would imagine, *à priori*, to be the most noxious of all kinds of air. For if common air only tainted with putrefaction be so deadly, much more might one expect that air to be so, which was generated from putrefaction only; but it seems to be nothing more than common air tainted with putrefaction, and therefore requires no other process to sweeten it. In this case, however, we seem to have an instance of the generation of genuine common air, though mixed with something that is foreign to it. Perhaps the residuum of fixed air may be another instance of the same nature.

Fixed air is equally diffused through the whole mass of any quantity of putrid air with which it is mixed; for dividing the mixture into two equal parts, they were reduced in the same proportion by passing through water. But this is also the case with some of the kinds of air which will not incorporate, as inflammable air, and air in which brimstone has burned.

If fixed air tend to correct air which has been injured by animal respiration or putrefaction, lime-kilns, which discharge great quantities of fixed air, may be wholesome in the neighbourhood of populous cities, the atmosphere of which must abound with putrid effluvia. I should think also that physicians might avail themselves of the application

of

of fixed air in many putrid disorders, especially as it may be so easily administered by way of clyster, where it would often find its way to much of the putrid matter. Nothing is to be apprehended from the distention of the bowels by this kind of air, since it is so readily absorbed by any fluid or moist substance. Since fixed air is not noxious *per se*, but, like fire, only in excess, I do not think it at all hazardous to attempt to breathe it. It is however easily conveyed into the stomach, in natural or artificial Pyrmont water, in briskly fermenting liquors, or a vegetable diet. It is possible, however, that a considerable quantity of fixed air might be imbibed by the absorbing vessels of the skin, if the whole body, except the head, should be suspended over a vessel of strongly fermenting liquor; and in some putrid disorders this treatment might be very salutary. If the body was exposed quite naked, there would be very little danger from the cold in this situation, and the air having freer access to the skin might produce a greater effect. Being no physician, I run no risk by throwing out these random, and perhaps whimsical, proposals.

Having communicated my observations on fixed air, and especially my scheme of applying it by way of *clyster* in putrid disorders, to Mr. Hey, an ingenious surgeon in this town, a case presently occurred, in which he had an opportunity of giving it a trial; and mentioning it to Dr. Hird and Dr. Crowther, two physicians who attended the patient, they approved the scheme, and it was put in execution: both by applying the fixed air by way of clyster, and at the same time making the

 patient

patient drink plentifully of liquors ſtrongly impregnated with it. The event was ſuch, that I requeſted Mr. Hey to draw up a particular account of the caſe, deſcribing the whole of the treatment, that the public might be ſatisfied that this new application of fixed air is perfectly ſafe, and alſo have an opportunity of judging how far it had the effect which I expected from it; and as the application is new, and not unpromiſing, I ſhall beg leave to ſubjoin his letter to me on the ſubject, by way of *Appendix* to theſe papers.

V.

Of Air in which a mixture of brimstone and filings of iron has stood.

Finding in Dr. Hales's account of his experiments, that there was a great diminution of the quantity of air in which a mixture of powdered brimſtone and filings of iron, made into a paſte with water, had ſtood, I repeated the experiment, and found the diminution greater than I had expected. The diminution of air by this proceſs is made as effectually, and as expeditiouſly, in quickſilver as in water; and it may be meaſured with the greateſt accuracy, becauſe there is neither any previous expanſion nor increaſe of the quantity of air, and becauſe it is ſome time before it begins to have any ſenſible effect. The diminution of air by this proceſs is various; but I have generally

generally found it to be between $\frac{1}{4}$ and $\frac{1}{5}$ of the whole.

Air thus diminiſhed is not heavier, but rather lighter than common air; and though lime-water does not become turbid when it is expoſed to this air, it is probably owing to the formation of a ſelenitic ſalt, as was the caſe with the ſimple burning of brimſtone abovementioned. That ſomething proceeding from the brimſtone ſtrongly affects the water which is confined in the ſame place with this brimſtone, is manifeſt from the very ſtrong ſmell that it has of the volatile ſpirit of vitriol. I conclude the diminution of air by this proceſs is of the ſame kind with the diminution of it in the other caſes, becauſe when this mixture is put into air which has been previouſly diminiſhed, either by the burning of candles, by reſpiration, or putrefaction, though it never fails to diminiſh it ſomething more, it is, however, no farther than this proceſs alone would have done it. If a freſh mixture be introduced into a quantity of air which had been reduced by a former mixture, it has little or no farther effect.

I obſerved, that when a mixture of this kind was taken out of a quantity of air in which a candle had before burned out, and in which it had ſtood for ſeveral days, it was quite cold and black, as it always becomes in a confined place; but it preſently grew very hot, ſmoaked copiouſly, and ſmelled very offenſively; and when it was cold, it was brown, like the ruſt of iron.

I once put a mixture of this kind to a quantity of inflammable air, made from iron, by which means it was diminiſhed $\frac{1}{9}$ or $\frac{1}{10}$ in its bulk; but, as far as I could

I could judge, it was ſtill as inflammable as ever. Another quantity of inflammable air was alſo reduced in the ſame proportion, by a mouſe putrefying in it; but its inflammability was not ſeemingly leſſened.

Air diminiſhed by this mixture of iron filings and brimſtone, is exceedingly noxious to animals, and I have not perceived that it grows any better by keeping in water. The ſmell of it is very pungent and offenſive.

The quantity of this mixture which I made uſe of in the preceding experiments, was from two to four ounce meaſures; but I did not perceive, but that the diminution of the quantity of air (which was generally about twenty ounce meaſures) was as great with the ſmalleſt, as with the largeſt quantity. How ſmall a quantity is neceſſary to diminiſh a given quantity of air to a *maximum*, I have made no experiments to aſcertain.

As ſoon as this mixture of iron filings, with brimſtone and water, begins to ferment, it alſo turns black, and begins to ſwell, and it continues to do ſo, till it occupies twice as much ſpace as it did at firſt; and the force with which it expands is great; but how great it is I have not endeavoured to determine.

When this mixture is immerſed in water, it generates no air, though it becomes black, and ſwells.

VI.

Of nitrous Air.

Ever ſince I firſt read Dr. Hales's moſt excellent Statical Eſſays, I was particularly ſtruck with that experiment of his, of which an account is given, Vol. I. p. 224, and Vol. II. p. 280; in which common air, and air generated from the Walton pyrites, by ſpirit of nitre, made a turbid red mixture, and in which part of the common air was abſorbed; but I never expected to have the ſatisfaction of ſeeing this remarkable appearance, ſuppoſing it to be peculiar to that particular mineral. Happening to mention this ſubject to the Hon. Mr. Cavendiſh, when I was in London, in the ſpring of the year 1772, he ſaid that he did not imagine but that other kinds of pyrites might anſwer as well as that which Dr. Hales made uſe of, and that probably the red appearance of the mixture depended upon the ſpirit of nitre only. This encouraged me to attend to the ſubject; and having no pyrites, I began with the ſolution of the different metals in ſpirit of nitre, and catching the air which was generated in the ſolution, I preſently found what I wanted, and a good deal more.

Beginning with the ſolution of braſs, on the 4th of June 1772, I firſt found this remarkable ſpecies of air; one effect of which, though it was caſually obſerved by Dr. Hales, he gave but little attention to; and which, as far as I know, has paſſed altogether unnoticed ſince his time, inſomuch that no name has been given to it. I therefore found myſelf, contrary to

to my first resolution, under an absolute necessity of giving a name to this kind of air myself. When I first began to speak and write of it to my friends, I happened to distinguish it by the name of nitrous air, because I had procured it by means of spirit of nitre only; and though I cannot say that I altogether like the term, because this air is not got from all the metals by the same spirit, neither myself nor any of my friends, to whom I have applied for the purpose, have been able to hit upon a better; so that I am obliged, after all, to content myself with it.

I have found that this kind of air is readily procured from iron, copper, brass, tin, silver, quicksilver, bismuth, and nickel, by the nitrous acid only, and from gold and the regulus of antimony by aqua regia. The circumstances attending the solution of each of these metals are various, but hardly worth mentioning, in treating of the properties of the air which they yield, which, from what metal soever it is extracted, has, as far as I have been able to observe, the very same properties.

One of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. The smell of it, also, is very strong, and remarkable, but very much resembling that of smoking spirit of nitre.

The diminution of a mixture of this and common air is not an equal diminution of both the kinds, which is all that Dr. Hales could observe, but of the common air chiefly, though not wholly. For if one measure of nitrous air be put to two measures of

 common

common air, in a few minutes (by which time the effervescence will be over, and the mixture will have recovered its transparency) there will want about one ninth of the original two measures. I hardly know any experiment that is more adapted to amaze and surprize than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is diminished by it. If, after this full saturation of common air with nitrous air, more nitrous air be put to it, it makes an addition equal to its own bulk, without producing the least redness, or any other visible effect.

That this diminution is chiefly in the quantity of common air, is evident from this observation, that if the smallest quantity of common air be put to any larger quantity of nitrous air, though the two together will not occupy so much space as they did separately, yet the quantity will be still larger than that of the nitrous air only. One ounce measure of common air being put to near twenty ounce measures of nitrous air, made an addition to it of about half an ounce measure. This, however, being a much greater proportion than the diminution of common air, in the former experiment, seems to prove that part of the diminution in the former case is in the nitrous air. Besides, it will presently appear, that nitrous air is subject to a most remarkable diminution; and as common air, in a variety of other cases, suffers a diminution from one fifth to one fourth, I conclude, that in this case also it does not exceed that proportion, and therefore that the remainder of the diminution respects the nitrous air.

In

In order to judge whether the water contributed tothe diminution of this mixture of nitrous and common air, I made the whole proceſs ſeveral times in quickſilver, uſing one third of nitrous, and two thirds of common air, as before. In this caſe the redneſs continued a very long time, and the diminution was not ſo great as when the mixtures had been made in water, there remaining one ſeventh more than the original quantity of common air. This mixture ſtood all night upon the quickſilver; and the next morning I obſerved that it was no farther diminiſhed upon the admiſſion of water to it, nor by pouring it ſeveral times through the water, and letting it ſtand in water two days. Another mixture, which ſtood about ſix hours on the quickſilver, was diminiſhed a little more upon the admiſſion of water, but was never leſs than the original quantity of common air. In another caſe, however, in which the mixture ſtood but a very ſhort time in quickſilver, the farther diminution, which took place upon the admiſſion of water, was much more conſiderable; ſo that the diminution, upon the whole, was very nearly as great as if the proceſs had been intirely in water. It is evident from theſe experiments, that the diminution is in part owing to the abſorption by the water; but that when the mixture is kept a long time, in a ſituation in which there is no water to abſorb any part of it, it acquires a conſtitution, by which it is afterwards incapable of being abſorbed by water.

In order to determine whether the fixed part of common air was depoſited in the diminution of it

by

by nitrous air, I inclosed a vessel full of lime water in the jar in which the process was made, but it occasioned no precipitation of the lime; and when the vessel was taken out, after it had been in that situation a whole day, the lime was easily precipitated by breathing into it as usual.

It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, or air fit for respiration; and, as far as I can judge, from a great number of observations, is at least very nearly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by putting mice, or any other animals, to breathe in it. This was a most agreeable discovery to me, as I hope it may be an useful one to the public; especially as, from this time, I had no occasion for so large a stock of mice as I had been used to keep for the purpose of these experiments, using them only in those which required to be very decisive; and in these cases I have seldom failed to know beforehand in what manner they would be affected.

It is also remarkable that, on whatever account air is unfit for respiration, this same test is equally applicable. Thus there is not the least effervescence between nitrous and fixed air, or inflammable air, or any species of diminished air. Also the degree of diminution being from nothing at all to more than one third of the whole of any quantity of air, we are by this means in possession of a prodigiously large scale, by which we may distinguish

very ſmall degrees of difference in the goodneſs of air. I have not attended much to this circumſtance, having uſed this teſt chiefly for greater differences; but, if I did not deceive myſelf, I have perceived a real difference in the air of my ſtudy, after a few perſons have been with me in it; and the air on the outſide of the houſe. Alſo a phial of air having been ſent me, from the neighbourhood of York, it appeared not to be ſo good as the air near Leeds; that is, it was not diminiſhed ſo much by an equal mixture of nitrous air, every other circumſtance being as nearly the ſame as I could contrive. It may perhaps be poſſible, but I have not yet attempted it, to diſtinguiſh ſome of the different winds, or the air of different times of the year, by this teſt.

By means of this teſt I was able to determine what I was before in doubt about, *viz.* the kind as well as the degree of injury done to air by candles burning in it. I could not tell with certainty by means of mice, whether it was at all injured with reſpect to reſpiration; and yet if nitrous air may be depended upon for furniſhing an accurate teſt, it muſt be rather more than one third worſe than common air, and have been diminiſhed by the ſame general cauſe of the other diminutions of air. For when, after many trials, I put one meaſure of thoroughly putrid and highly noxious air, into the ſame veſſel with two meaſures of good wholeſome air, and into another veſſel an equal quantity, *viz.* three meaſures of air in which a candle had burned out; and then put equal quantities of nitrous air to each of them, the former was diminiſhed rather more than the latter. It agrees

with

with this obſervation, that burned air is farther diminiſhed both by putrefaction, and a mixture of iron filings and brimſtone; and I therefore, take it for granted, by every other cauſe of the diminution of air. It is probable, therefore, that burned air is air ſo far loaded with phlogiſton, as to be able to extinguiſh a candle, which it may do long before it is fully ſaturated.

Inflammable air with a mixture of nitrous air burns with a green flame. This makes a very pleaſing experiment when it is properly conducted. As, for ſome time, I chiefly made uſe of copper for the generation of nitrous air, I firſt aſcribed this circumſtance to that property of this metal, by which it burns with a green flame; but I was preſently ſatisfied that it muſt ariſe from the ſpirit of nitre, for the effect is the very ſame from whichever of the metals the nitrous air is extracted, all of which I tried for this purpoſe, even ſilver and gold. A mixture of oil of vitriol and ſpirit of nitre in equal proportions diſſolved iron, and the produce was nitrous air; but a leſs degree of ſpirit of nitre in the mixture produced air that was inflammable, and which burned with a green flame. It alſo tinged common air a little red, and diminiſhed it, though not much.

The diminution of common air by a mixture of nitrous air, is not ſo extraordinary as the diminution which nitrous air itſelf is ſubject to from a mixture of iron filings and brimſtone, made into a paſte with water. This mixture, as I have already obſerved, diminiſhes common air between one fifth and one fourth, but has no ſuch effect upon

any kind of air that has been diminifhed, and rendered noxious by any other procefs; but when it is put to a quantity of nitrous air, it diminifhes it fo much, that no more than one fourth of the original quantity will be left. The effect of this procefs is generally perceived in five or fix hours, about which time the vifible effervefcence of the mixture begins; and in a very fhort time it advances fo rapidly, that in about an hour almoft the whole effect will have taken place. If it be fuffered to ftand a day or two longer, the air will ftill be diminifhed farther, but only a very little farther, in proportion to the firft diminution. The glafs jar, in which the air and this mixture have been confined, has generally been fo much heated in this procefs, that I have not been able to touch it.

Nitrous air thus diminifhed has not the peculiar fmell of nitrous air, but fmells juft like common air in which the fame mixture has ftood; and it is not capable of being diminifhed any farther, by a frefh mixture of iron and brimftone.

Common air faturated with nitrous air is alfo no farther diminifhed by this mixture of iron filings and brimftone, though the mixture ferments with great heat, and fwells very much in it.

Plants die very foon, both in nitrous air, and alfo in common air faturated with nitrous air, but efpecially in the former.

Neither nitrous air, nor common air faturated with nitrous air, differs in fpecific gravity from common air, or, at leaft, fo little, that I could

 not

not be ſure of it, ſometimes about three pints of it ſeeming to be about half a grain heavier, and at other times as much lighter than common air.

Having, among other kinds of air, expoſed a quantity of nitrous air, to water out of which the air had been well boiled, in the experiment to which I have more than once referred, as having been the occaſion of ſeveral new and important obſervations, I found that $\frac{19}{20}$ of the whole was abſorbed. Perceiving, to my great ſurprize, that ſo very great a proportion of this kind of air was miſcible with water, I immediately began to agitate a conſiderable quantity of it, in a jar ſtanding in a trough of the ſame kind of water; and with about four times as much agitation as fixed air requires, it was ſo far abſorbed by the water, that only about one fifth remained. This remainder extinguiſhed flame, and was noxious to animals. Afterwards I diminiſhed a pretty large quantity of it to one eighth of its original bulk, and the remainder ſtill retained much of its peculiar ſmell, and diminiſhed common air a little. A mouſe alſo died in it, but not ſo ſuddenly as it would have done in pure nitrous air. In this operation the peculiar ſmell of nitrous air is very manifeſt, the water being firſt impregnated with the air, and then tranſmitting it to the common atmoſphere.

This experiment gave me the hint of impregnating water with nitrous air, in the manner in which I had before done it with fixed air; and I preſently found that diſtilled water would imbibe about one tenth of its bulk of this kind of air, and

that

that it acquired a remarkably acid and aſtringent taſte from it. The ſmell of water thus impregnated is at firſt peculiarly pungent. I did not chuſe to ſwallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in ſome caſes, ſalutary.

This kind of air is retained very obſtinately by water. In an exhauſted receiver a quantity of water thus ſaturated emitted a whitiſh fume, ſuch as ſometimes iſſues from bubbles of this air when it is firſt generated, and alſo ſome air bubbles; but though it was ſuffered to ſtand a long time in this ſituation, it ſtill retained its peculiar taſte; but when it had ſtood all night pretty near the fire, the water was become quite vapid, and had depoſited a filmy kind of matter, of which I had often collected a conſiderable quantity from the trough in which jars containing this air had ſtood. This I ſuppoſe to be a precipitate of the metal by the ſolution of which the nitrous air was generated. I have not given ſo much attention to it as to know, with certainty, in what circumſtances this depoſit is made, any more than I do the matter depoſited from inflammable air abovementioned; for I cannot get it, at leaſt in any conſiderable quantity, when I pleaſe; whereas I have often found abundance of it, when I did not expect it at all.

The nitrous air with which I made the firſt impregnation of water was extracted from copper; but when I made the impregnation with air from quickſilver, the water had the very ſame taſte, though the matter depoſited from it ſeemed to be of a dif-

 ferent

ferent kind; for it was whitish, whereas the other had a yellowish tinge. Except the first quantity of this impregnated water, I could never deprive any more that I made of its peculiar taste. I have even let some of it stand more than a week, in phials with their mouths open, and sometimes very near the fire, without producing any alteration in it.

Whether any of the spirit of nitre be properly contained in the nitrous air, and be mixed with the water in this operation, I have not yet endeavoured to determine. This, however, may probably be the case, as the spirit of nitre is in a considerable degree volatile.

It will perhaps be thought, that the most useful, if not the most remarkable, of all the properties of this extraordinary kind of air, is its power of preserving animal substances from putrefaction, and of restoring those that are already putrid, which it possesses in a far greater degree than fixed air. My first observation of this was altogether casual. Having found nitrous air to suffer so great a diminution as I have already mentioned by a mixture of iron filings and brimstone, I was willing to try whether it would be equally diminished by other causes of the diminution of common air, especially by putrefaction; and for this purpose I put a dead mouse into a quantity of it, and placed it near the fire, where the tendency to putrefaction was very great. In this case there was a considerable diminution, *viz.* from $5\frac{1}{2}$ to $3\frac{1}{2}$; but not so great as I had expected, the antiseptic power of the nitrous air having checked

the

the tendency to putrefaction; for when, after a week, I took the mouſe out, I perceived, to my very great ſurprize, that it had no offenſive ſmell.

Upon this I took two other mice, one of them juſt killed, and the other ſoft and putrid, and put them both into the ſame jar of nitrous air, ſtanding in the uſual temperature of the weather, in the months of July and Auguſt of 1772; and after 25 days, having obſerved that there was little or no change in the quantity of the air, I took the mice out; and, examining them, found them both perfectly ſweet, even when cut through in all places. That which had been put into the air when juſt dead was quite firm; and the fleſh of the other, which had been putrid and ſoft, was ſtill ſoft, but perfectly ſweet.

In order to compare the antiſeptic power of this kind of air with that of fixed air, I examined a mouſe which I had incloſed in a phial full of fixed air, as pure as I could make it, and which I had corked very cloſe; but upon opening this phial in water, about a month after, I perceived that a large quantity of putrid effluvium had been generated; for it ruſhed with violence out of the phial; and the ſmell that came from it, the moment the cork was taken out, was inſufferably offenſive. Indeed Dr. Macbride ſays, that he could only reſtore very thin pieces of putrid fleſh by means of fixed air. Perhaps the antiſeptic power of theſe kinds of air may be in proportion to their acidity. If a little pains were taken with this ſubject, this remarkable antiſeptic power of nitrous air might poſſibly be applied to various uſes, perhaps to the

preſervation

preſervation of the more delicate birds, fiſhes, fruits, &c. mixing it in different proportions with common or fixed air. Of this property of nitrous air anatomiſts may perhaps avail themſelves, as animal ſubſtances may by this means be preſerved in their natural ſoft ſtate; but how long it will anſwer for this purpoſe, experience only can ſhew.

I calcined lead and tin in the manner hereafter deſcribed in a quantity of nitrous air, but with very little ſenſible effect; which rather ſurprized me; as, from the reſult of the experiment with the iron filings and brimſtone, I had expected a very great diminution of the nitrous air by this proceſs, the mixture of iron filings and brimſtone, and the calcination of metals, having the ſame effect upon common air, both of them diminiſhing it in nearly the ſame proportion.

Nitrous air is procured from all the proper metals by ſpirit of nitre, except lead, and from all the ſemi-metals that I have tried, except zinc. For this purpoſe I have uſed biſmuth and nickel, with ſpirit of nitre only, and regulus of antimony and platina, with aqua regia.

I got little or no air from lead by ſpirit of nitre, and have not yet made any experiments to aſcertain the nature of this ſolution. With zinc I have taken a little pains.

Four penny weights and ſeventeen grains of zinc diſſolved in ſpirit of nitre, to which as much water was added, yielded about twelve ounce meaſures of air, which had, in ſome degree, the properties of nitrous air, making a ſlight efferveſcence with common air, and diminiſhing it about as much as nitrous

trous air, which had been itſelf diminiſhed one half by waſhing in water. The ſmell of them both was alſo the ſame; ſo that I concluded it to be the ſame thing, that part of the nitrous air which is imbibed by water being retained in this ſolution.

In order to diſcover whether this was the caſe, I made the ſolution boil in a ſand heat. Some air came from it in this ſtate, which ſeemed to be the ſame thing, as nitrous air diminiſhed about one ſixth, or one eighth, by waſhing in water. When the fluid part was evaporated, there remained a brown fixed ſubſtance, which was obſerved by Mr. Hellot, who deſcribes it, Ac. Par. 1735, M. p. 35. A part of this I threw into a ſmall red hot crucible; and covering it immediately with a receiver, ſtanding in water, I obſerved that very denſe red fumes roſe from it, and filled the receiver. This redneſs continued about as long as that which is occaſioned by a mixture of nitrous and common air; the air was alſo conſiderably diminiſhed within the receiver. This ſubſtance, therefore, muſt certainly have contained within it the very ſame thing, or principle, on which the peculiar properties of nitrous air depend. It is remarkable, however, that though the air within the receiver was diminiſhed about one fifth by this proceſs, it was itſelf as much affected with a mixture of nitrous air, as common air is, and a candle burnt in it very well. This may perhaps be attributed to ſome effect of the ſpirit of nitre, in the compoſition of that brown ſubſtance.

Nitrous air, I find, will be conſiderably diminiſhed in its bulk by ſtanding a long time in water,

ter, about as much as inflammable air is diminished in the same circumstances. For this purpose I kept for some months a quart bottle full of each of these kinds of air; but as different quantities of inflammable air vary very much in this respect, it is not improbable but that nitrous air may vary also.

From one trial that I made, I conclude that nitrous air may be kept in a bladder much better than most other kinds of air. The air to which I refer was kept about a fortnight in a bladder, through which the peculiar smell of the nitrous air was very sensible for several days. In a day or two the bladder became red, and was much contracted in its dimensions. The air within it had lost very little of its peculiar property of diminishing common air.

I did not endeavour to ascertain the exact quantity of nitrous air produced from given quantities of all the metals which yield it; but the few observations which I did make for this purpose I shall recite in this place:

dwt.	gr.			
6	0	of silver yielded	17¾	ounce measures
5	19	of quicksilver	4½	
1	2½	of copper	14½	
2	0	of brass	21	
0	20	of iron	16	
1	5	of bismuth	6	
0	12	of nickel	4	

VII. Of

VII.

OF AIR INFECTED WITH THE FUMES OF BURNING CHARCOAL.

Air infected with the fumes of burning charcoal is well known to be noxious; and the Honourable Mr. Cavendish favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 ounce measures, by passing through a red-hot iron tube filled with the dust of charcoal. This diminution he ascribed to such a destruction of common air as Dr. Hales imagined to be the consequence of burning. Mr. Cavendish also observed, that there had been a generation of fixed air in this process, but that it was absorbed by sope leys. This experiment I also repeated, with a small variation of circumstances, and with nearly the same result.

Afterwards, I endeavoured to ascertain, by what appears to me to be an easier and a more certain method, in what manner air is affected with the fumes of charcoal, viz. by suspending bits of charcoal within glass vessels, filled to a certain height with water, and standing inverted in another vessel of water, while I threw the focus of a burning mirror, or lens, upon them. In this manner I diminished a given quantity of air one fifth, which is nearly in the same proportion with other diminutions of air.

Some fixed air seems to be contained in charcoal, and to be set loose from it by this process; for if I made use of lime-water, it never failed to become

 turbid,

turbid, presently after the heat was applied. This was the case with whatever degree of heat the charcoal had been made. If, however, the charcoal had not been made with a very considerable degree of heat, there never failed to be a permanent addition of inflammable air produced; which agrees with what I observed before, that, in converting dry wood into charcoal, the greatest part is changed into inflammable air. I have sometimes found, that charcoal which was made with the most intense heat of a smith's fire, which vitrified part of a common crucible in which the charcoal was confined, and which had been continued above half an hour, did not diminish the air in which the focus of a burning mirror was thrown upon it; a quantity of inflammable air equal to the diminution of the common air being generated in the process; whereas, at other times, I have not perceived that there was any generation of inflammable air, but a perfect diminution of common air, when the charcoal had been made with a much less degree of heat. This subject deserves to be farther investigated.

To make the preceding experiment with still more accuracy, I repeated it in quicksilver; when I perceived that there was a small increase of the quantity of air, from a generation either of fixed or inflammable air, but I suppose of the former. Thus it stood without any alteration a whole night, and part of the following day; when lime-water, being admitted to it, it presently became turbid, and, after some time, the whole quantity of air, which was about four ounce measures, was diminished one fifth, as before. In this case, I carefully weighed the piece of charcoal, which was exactly two grains, and could not find

that

that it was ſenſibly diminiſhed in weight by the operation.

Air thus diminiſhed by the fumes of burning charcoal not only extinguiſhes flame, but is in the higheſt degree noxious to animals; it makes no efferveſcence with nitrous air, and is incapable of being diminiſhed any farther by the fumes of more charcoal, by a mixture of iron filings and brimſtone, or by any other cauſe of the diminution of air that I am acquainted with.

This obſervation, which reſpects all other kinds of diminiſhed air, proves that Dr. Hales was miſtaken in his notion of the abſorption of air in thoſe circumſtances in which he obſerved it. For he ſuppoſed that the remainder was, in all caſes, of the ſame nature with that which had been abſorbed, and that the operation of the ſame cauſe would not have failed to produce a farther diminution; whereas all my obſervations not only ſhew that air, which has once been fully diminiſhed by any cauſe whatever, is not only incapable of any farther diminution, either from the ſame or from any other cauſe, but that it has likewiſe acquired new properties, moſt remarkably different from thoſe which it had before, and that they are, in a great meaſure, the ſame in all the caſes. Theſe circumſtances give reaſon to ſuſpect, that the cauſe of diminution is, in reality, the ſame in all the caſes. What this cauſe is, may, perhaps, appear in the next courſe of obſervations.

VIII.

Of the effect of the calcination of metals, and of the effluvia of paint made with white-lead and oil, on Air.

Having been led to ſuſpect, from the experiments which I had made with charcoal, that the diminution of air in that caſe, and perhaps in other caſes alſo, was, in ſome way or other, the conſequence of its having more than its uſual quantity of phlogiſton, it occurred to me, that the calcination of metals, which are generally ſuppoſed to conſiſt of nothing but a metallic earth united to phlogiſton, would tend to aſcertain the fact, and be a kind of *experimentum crucis* in the caſe. Accordingly, I ſuſpended pieces of lead and tin in given quantities of air, in the ſame manner as I had before treated the charcoal; and throwing the focus of a burning mirror or lens upon them, in ſuch a manner as to make them fume copiouſly, I preſently perceived a diminution of the air. In the firſt trial that I made, I reduced four ounce meaſures of air to three, which is the greateſt diminution of common air that I had ever obſerved before, and which I account for, by ſuppoſing that, in other caſes, there was not only a cauſe of diminution, but cauſes of addition alſo, either of fixed or inflammable air, or ſome other permanently elaſtic matter, but that, the effect of the calcination of metals being ſimply the eſcape of phlogiſton, the cauſe of diminution was alone and uncontrouled.

The

The air, which I had thus diminiſhed by calcination of lead, I transferred into another clean phial, but found that the calcination of more lead in it had no farther effect upon it. This air alſo, like that which had been infected with the fumes of charcoal, was in the higheſt degree noxious, made no effervefcence with nitrous air, was no farther diminiſhed by the mixture of iron filings and brimſtone, and was not only rendered innoxious, but alſo recovered, in a great meaſure, the other properties of common air, by waſhing in water.

It might be ſuſpected that the noxious quality of the air in which lead was calcined, might be owing to ſome fumes peculiar to that metal; but I found no ſenſible difference between the properties of this air, and that in which tin was calcined.

The water over which metals are calcined acquires a yellowiſh tinge, and an exceedingly pungent ſmell and taſte, pretty much, as near as I can recollect, for I did not compare them together, like that over which brimſtone has been frequently burned. Alſo a thin and whitiſh pellicle covered both the ſurface of the water, and likewiſe the ſides of the phial in which the calcination was made, inſomuch that, without frequently agitating the water, it grew ſo opaque by this conſtantly accumulating incruſtation, that the ſun beams could not be tranſmitted through it in a quantity ſufficient to produce the calcination.

I imagined, however, that, even when this air was transferred into a clean phial, the metals were not ſo eaſily melted or calcined as they were in freſh air; for the air being once fully ſaturated with phlogiſton, may not ſo readily admit any more, though it be only

to tranfmit it to the water. I alfo fufpected that metals were not eafily melted or calcined in inflammable, fixed, or nitrous, air, or any kind of diminifhed air. None of thefe kinds of air fuffered any change by this operation; nor was there any precipitation of lime, when charcoal was heated in any of thefe kinds of air ftanding in lime-water.

Query. May not water impregnated with phlogifton from calcined metals, or by any other method, be of fome ufe in medicine? The effect of this impregnation is exceedingly remarkable; but the principle with which it is impregnated is volatile, and entirely efcapes in a day or two, if the furface of the water be expofed to the common atmofphere.

It fhould feem that phlogifton is retained more obftinately by charcoal than it is by lead or tin; for when any given quantity of air is fully faturated with phlogifton from charcoal, no heat that I have yet applied has been able to produce any more effect upon it; whereas, in the fame circumftances, lead and tin may ftill be calcined. The air, indeed, can take no more; but the water receives it, and the fides of the phial alfo receive an addition of incruftation. This is a white powdery fubftance, and well deferves to be examined. I fhall endeavour to do it at my leifure.

Lime-water never became turbid by the calcination of metals over it; but the colour, fmell, and tafte of the water was always changed, and the furface of it became covered with a yellow pellicle, as before.

When this procefs was made in quickfilver, the air was diminifhed only one fifth; and upon water being

admitted to it, no more was abſorbed; which is an effect ſimilar to that of a mixture of nitrous and common air, which was mentioned before.

The preceding experiments on the calcination of metals ſuggeſted to me a method of explaining the cauſe of the miſchief which is known to ariſe from freſh paint, made with white lead (which I ſuppoſe is an imperfect calx of lead) and oil. To verify my hypotheſis, I firſt put a ſmall pot full of this kind of paint, and afterwards (which anſwered much better, by expoſing a greater ſurface of the paint) I daubed ſeveral pieces of paper with it, and put them under a receiver, and obſerved, that in about twenty-four hours, the air was diminiſhed between one fifth and one fourth, for I did not meaſure it very exactly. This air alſo was, as I expected to find it, in the higheſt degree, noxious; it did not effervеſce with nitrous air, it was no farther diminiſhed by a mixture of iron filings and brimſtone, and was made wholeſome by agitation in water deprived of all air.

I think it appears pretty evident, from the preceding experiments on the calcination of metals, that air is ſome way or other diminiſhed in conſequence of being highly charged with phlogiſton, and that agitation in water reſtores it, by imbibing a great part of the phlogiſtic matter. That water has a conſiderable affinity with phlogiſton, is evident from the ſtrong impregnation which it receives from it. May not plants alſo reſtore air diminiſhed by putrefaction, by abſorbing part of the phlogiſton with which it is loaded? The greater part of a dry plant, as well as of a dry animal ſubſtance, conſiſts of inflammable air, or ſomething that is capable of being converted

into

into inflammable air; and it seems to be as probable that this phlogistic matter may have been imbibed by the roots and leaves of plants, and afterwards incorporated into their substance, as that it is altogether produced by the power of vegetation. May not this phlogistic matter be even the most essential part of the food and support of both vegetable and animal bodies?

In the experiments with metals, the diminution of air seems to be the consequence of nothing but a saturation with phlogiston; and in all the other cases of the diminution of air, I do not see but that it may be effected by the same means. When a vegetable or animal substance is dissolved by putrefaction, the escape of the phlogistic matter (which, together with all its other constituent parts, is then let loose from it) may be the circumstance that produces the diminution of the air in which it putrefies. It is highly improbable that what remains after an animal body has been thoroughly dissolved by putrefaction, should yield so great a quantity of inflammable air, as the dried animal substance would have done. Of this I have not made an actual trial, though I have often thought of doing it, and still intend to do it; but I think there can be no doubt of the result. Again, the iron, by its fermentation with brimstone and water, is evidently reduced to a calx, so that phlogiston must have escaped from it. Phlogiston also must evidently be set loose by the ignition of charcoal, and is not improbably the matter which flies off from paint, composed of white lead and oil. Lastly, since spirit of nitre is known to have a very remarkable affinity with phlogiston, it is far from being

being improbable that nitrous air may alſo produce the ſame effect by the ſame means.

To this hypotheſis it may be objected, that, if diminiſhed air be air ſaturated with phlogiſton, it ought to be inflammable; but this by no means follows, ſince its inflammability may depend upon ſome particular mode of combination, or degree of affinity, with which we are not acquainted. Beſides, inflammable air ſeems to conſiſt of ſome other principle, or to have ſome other conſtituent part, beſides phlogiſton and common air, as is probable from that remarkable depoſit, which, as I have obſerved, is made by inflammable air, both from iron and zinc.

It is not improbable, however, but that a greater degree of heat may inflame that air which extinguiſhes a common candle, if it could be conveniently applied. Air that is inflammable, I obſerve, extinguiſhes red hot wood; and indeed inflammable ſubſtances can only be thoſe which, in a certain degree of heat, have a leſs affinity with the phlogiſton they contain, than the air, or ſome other contiguous ſubſtance, has with it; ſo that the phlogiſton only quits one ſubſtance, with which it was before combined, and enters another, with which it may be combined in a very different manner. This ſubſtance, however, whether it be air or any thing elſe, being now fully ſaturated with phlogiſton, and not being able to take any more, in the ſame circumſtances, muſt neceſſarily extinguiſh fire, and put a ſtop to the ignition of all other bodies, that is, to the farther eſcape of phlogiſton from them.

That plants reſtore noxious air, by imbibing the phlogiſton with which it is loaded, is very agreeable to

 the

the conjectures of Dr. Franklin, made many years ago, and expressed in the following extract from the last edition of his Letters, p. 346.

" I have been inclined to think that the fluid *fire*, " as well as the fluid *air*, is attracted by plants in " their growth, and becomes consolidated with the " other materials of which they are formed, and " makes a great part of their substance; that, when " they come to be digested, and to suffer in the " vessels a kind of fermentation, part of the fire, as " well as part of the air, recovers its fluid active state " again, and diffuses itself in the body, digesting and " separating it; that the fire so reproduced, by di- " gestion and separation, continually leaving the " body, its place is supplied by fresh quantities, " arising from the continual separation; that what- " ever quickens the motion of the fluids in an ani- " mal quickens the separation, and re-produces " more of the fire, as exercise; that all the fire " emitted by wood, and other combustibles, when " burning, existed in them before, in a solid state, " being only discovered when separating; that some " fossils, as sulphur, sea-coal, &c. contain a great " deal of solid fire; and that, in short, what escapes " and is dissipated in the burning of bodies, besides " water and earth, is generally the air, and fire, " that before made parts of the solid."

IX.

Of Air procured by means of spirit of salt.

Being very much struck with the result of an experiment of the Hon. Mr. Cavendish, related Phil. Trans.

Tranſ. Vol. LVI. p. 157. by which, though, he ſays, he was not able to get any inflammable air from copper, by means of ſpirit of ſalt, he got a much more remarkable kind of air, *viz.* one that loſt its elaſticity by coming into contact with water, I was exceedingly deſirous of making myſelf acquainted with it. On this account, I began with making the experiment in quickſilver, which I never failed to do in any caſe in which I ſuſpected that air might either be abſorbed by water, or be in any other manner affected by it; and by this means I preſently got a much more diſtinct idea of the nature and effects of this curious ſolution.

Having put ſome copper filings into a ſmall phial, with a quantity of ſpirit of ſalt; and making the air, which was generated in great plenty, on the application of heat, aſcend into a tall glaſs veſſel full of quickſilver, and ſtanding in quickſilver, the whole produce continued a conſiderable time without any change of dimenſions. I then introduced a ſmall quantity of water to it, when about three fourths of it (the whole being about four ounce meaſures) preſently, but gradually, diſappeared, the quickſilver riſing in the veſſel. I then introduced a conſiderable quantity of water; but there was no farther diminution of the air, and the remainder I found to be inflammable.

Having frequently continued this proceſs a long time after the admiſſion of the water, I was much amuſed with obſerving the large bubbles of the newly generated air, which came through the quickſilver, the ſudden diminution of them when they came to the water, and the very ſmall bubbles which went

 through

through the water. They made, however, a continual, though ſlow, increaſe of inflammable air.

Fixed air, being admitted to the whole produce of this air from copper, had no ſenſible effect upon it. Upon the admiſſion of water, a great part of the mixture, which, no doubt, was the moſt ſubtle kind of air from the copper, preſently diſappeared; another part, which I ſuppoſe to have been the fixed air, was abſorbed ſlowly; and in this particular caſe the very ſmall permanent reſiduum did not take fire; but it is very poſſible that it might have done ſo, if the quantity had been greater.

Lime-water being admitted to the whole produce of air from copper became white; but this I ſuſpect to have ariſen from ſome other circumſtance than the precipitation of the lime which it contained.

The ſolution of lead in the marine acid is attended with the very ſame phænomena as the ſolution of copper in the ſame acid; about three fourths of the generated air diſappearing on the contact of water, and the remainder being inflammable.

The ſolutions of iron, tin, and zinc, in the marine acid, were all attended with the ſame phænomena as the ſolutions of copper and lead, but in a leſs degree; for in iron one eighth, in tin one ſixth, and in zinc one tenth of the generated air diſappeared on the contact with water. The remainder of the air from iron, in this caſe, burned with a green, or very light blue flame.

I had always thought it ſomething extraordinary that a ſpecies of air ſhould loſe its elaſticity by the mere contact of any thing, and from the firſt ſuſpected that it muſt have been imbibed by the water

that

that was admitted to it; but ſo very great a quantity of this air diſappeared upon the admiſſion of a very ſmall quantity of water, that I could not help concluding that appearances favoured the former hypotheſis. I found, however, that when I admitted a much ſmaller quantity of water, confined in a narrow glaſs tube, a part only of the air diſappeared, and that very ſlowly, and that more of it vaniſhed upon the admiſſion of more water. This obſervation put it beyond a doubt, that this air was properly imbibed by the water, which, being once fully ſaturated with it, was not capable of receiving any more. The water thus impregnated taſted very acid, even when it was much diluted with other water, through which the tube containing it was drawn. It even diſſolved iron very faſt, and generated inflammable air. This laſt obſervation, together with another which immediately follows, led me to the diſcovery of the true nature of this remarkable kind of air, as it had hitherto been called.

Happening, at one time, to uſe a good deal of copper and a ſmall quantity of ſpirit of ſalt, in the generation of this kind of air, I was ſurprized to find that air was produced long after, I could not but think that the acid muſt have been ſaturated with the metal; and I alſo found that the proportion of inflammable air to that which was abſorbed by the water continually diminiſhed, till, inſtead of being one fourth of the whole as I had firſt obſerved, it was not ſo much as one twentieth. Upon this, I concluded that this ſubtle air did not ariſe from the copper, but from the ſpirit of ſalt; and preſently making the experiment with the acid only, without any cop-

per, or metal of any kind, this air was immediately produced in as great plenty as before; ſo that this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of ſpirit of ſalt, which appear to be of ſuch a nature, that they are not liable to be condenſed by cold, like the vapour of water, and other fluids. This vapour, however, ſeems to loſe its elaſticity, in ſome meaſure, gradually, unleſs it ſhould be thought to be affected by the quickſilver, with which it is in contact; for it was always diminiſhed, more or leſs, by ſtanding.

This elaſtic acid vapour extinguiſhes flame, and is much heavier than common air; but how much heavier, will not be eaſy to aſcertain. A cylindrical glaſs veſſel, about three fourths of an inch in diameter, and four inches deep, being filled with it, and turned upſide down, a lighted candle may be let down into it more than twenty times before it will burn at the bottom. It is pleaſing to obſerve the colour of the flame in this experiment; for both before the candle goes out, and alſo when it is firſt lighted again, it burns with a beautifully green, or rather light blue flame, ſuch as is ſeen when common ſalt is thrown into the fire.

When this elaſtic vapour is all expelled from any quantity of ſpirit of ſalt, which is eaſily perceived by the vapour being condenſed by cold, the remainder is a very weak acid, barely capable of diſſolving iron.

Being now in the poſſeſſion of a new ſubject of experiments, *viz.* an elaſtic acid vapour, in the form of a permanent air, eaſily procured, and effectually confined by glaſs and quickſilver, with which

which it did not ſeem to have any affinity; I immediately began to introduce a variety of ſubſtances to it, in order to aſcertain its peculiar properties and affinities, and alſo the properties of thoſe other bodies with reſpect to it.

Beginning with water, which, from preceding obſervations, I knew would imbibe it, and become impregnated with it; I found that 2½ grains of rain water abſorbed three ounce meaſures of this vapour, after which it was increaſed one third in its bulk, and weighed twice as much as before; ſo that this concentrated vapour ſeems to be twice as heavy as rain water. Water impregnated with it makes the ſtrongeſt ſpirit of ſalt that I have ſeen, diſſolving iron with the moſt rapidity. Conſequently, two thirds of the beſt ſpirit of ſalt is nothing more than mere phlegm or water.

Iron filings, being admitted to this vapour, were diſſolved by it pretty faſt, half of the vapour diſappearing, and the other half becoming inflammable air, not abſorbed by water. Putting chalk to it, fixed air was produced.

I had not introduced many ſubſtances to this vapour, before I diſcovered that it had an affinity with phlogiſton, ſo that it would deprive other ſubſtances of it, and form with it ſuch an union as conſtitutes inflammable air; which ſeems to ſhew, that inflammable air univerſally conſiſts of the union of ſome acid vapour with phlogiſton.

Inflammable air was produced, when to this vapour I put ſpirit of wine, oil of olives, oil of turpentine, charcoal, phoſphorus, bees-wax, and even ſulphur. This laſt obſervation, I own, ſur-

prized

prized me; for, the marine acid being reckoned the weakeſt of the three mineral acids, I did not think that it had been capable of diſlodging the oil of vitriol from this ſubſtance; but I found that it had the very ſame effect both upon alum and nitre; the vitriolic acid in the former caſe, and the nitrous in the latter, giving place to the ſtronger vapour of ſpirit of ſalt.

The ruſt of iron, and the precipitate of nitrous air made from copper, alſo imbibed this vapour very faſt, and the little that remained of it was inflammable air; which proves, that theſe calces contain phlogiſton. It ſeems alſo to be pretty evident, from this experiment, that the precipitate above-mentioned is a real calx of the metal, by the ſolution of which the nitrous air is generated.

As ſome remarkable circumſtances attend the abſorption of this vapour of ſpirit of ſalt, by the ſubſtances above-mentioned, I ſhall briefly mention them.

Spirit of wine abſorbs this vapour as readily as water itſelf, and is increaſed in bulk by that means. Alſo, when it is ſaturated, it diſſolves iron with as much rapidity, and ſtill continues inflammable.

Oil of olives abſorbs this vapour very ſlowly, and, at the ſame time, it turns almoſt black, and becomes glutinous. It is alſo leſs miſcible with water, and acquires a very diſagreeable ſmell. By continuing upon the ſurface of the water, it became white, and its offenſive ſmell went off in a few days.

Oil of turpentine abſorbed this vapour very faſt, turning brown, and almoſt black. No inflammable air was formed, till I raiſed more of the vapour than

the oil was able to abſorb, and let it ſtand a conſiderable time; and ſtill the air was but weakly inflammable. The ſame was the caſe with the oil of olives, in the laſt mentioned experiment; and it ſeems to be probable, that, the longer this acid vapour had continued in contact with the oil, the more phlogiſton it would have extracted from it. It is not improbable, but that, in the intermediate ſtate, before it becomes inflammable air, it may be nearly of the nature of common air.

Bees-wax abſorbed this vapour very ſlowly. About the bigneſs of a hazel-nut of the wax being put to three ounce meaſures of the vapour, the vapour was diminiſhed one half in two days, and, upon the admiſſion of water, half of the remainder alſo diſappeared. This air was ſtrongly inflammable.

Charcoal abſorbed this vapour very faſt. About one fourth of it was rendered immiſcible in water, and was but weakly inflammable.

A ſmall bit of phoſphorus, perhaps about half a grain, ſmoked, and gave light in the vapour of ſpirit of ſalt, juſt as it would have done in common air confined. It was not ſenſibly waſted after continuing about twelve hours in that ſtate, and the bulk of the vapour was very little diminiſhed. Water being admitted to it abſorbed it as before, except about one fifth of the whole, which was but weakly inflammable.

Putting ſeveral pieces of ſulphur to this vapour, it was abſorbed but ſlowly. In about twenty-four hours about one fifth of the quantity had diſappeared; and water being admitted to the remainder, very little

more was abſorbed. The remainder was inflammable, and burned with a blue flame.

Nowithſtanding the affinity which this vapour of ſpirit of ſalt appears to have with phlogiſton, it is not capable of depriving all bodies of it. I found that dry wood, cruſts of bread, and raw fleſh, very readily imbibed this acid vapour, but did not part with any of their phlogiſton to it. All theſe ſubſtances turned very brown, after they had been ſome time expoſed to this vapour, and taſted very ſtrongly of the acid when they were taken out; but the fleſh, when waſhed in water, became very white, and the fibres eaſily ſeparated from one another, even more than they would have done if it had been boiled or roaſted.

When I put a piece of ſaltpetre to this vapour, it was preſently ſurrounded with a white fume, which ſoon filled the whole veſſel, exactly like the fume which burſts from the bubbles of nitrous air, when it is generated by a vigorous fermentation, and ſuch as is ſeen when nitrous air is mixed with this vapour of ſpirit of ſalt. In about a minute, the whole quantity of vapour was abſorbed, except a very ſmall quantity, which might be the common air that had lodged upon the ſurface of the ſpirit of ſalt within the phial.

A piece of alum expoſed to this vapour turned yellow, abſorbed it as faſt as the ſaltpetre had done, and was reduced by it to the form of a powder. The ſurface both of the nitre and alum was, I doubt not, changed into common ſalt, by this proceſs. Common ſalt, as might be expected, had no effect whatever on this vapour.

From

From conſidering the affinity which this vapour has with phlogiſton, I was induced to try the effect of a mixture of it with nitrous air. Accordingly, to two parts of this vapour, I put one part of nitrous air, and, in about twenty-four hours, the whole was diminiſhed to ſomething leſs than the original quantity of the vapour, and was no farther diminiſhed by the admiſſion of water. Holding the flame of a candle over this air, the lower part of it burned green, but there was no ſenſible exploſion. At different times I collected 2¾ ounce meaſures of this mixture of air; but, upon agitating it in rain-water, it was preſently diminiſhed to 1½ ounce meaſures. In this ſtate it effervesced with nitrous air, and was conſiderably diminiſhed by it, but not ſo much as common air. Some allowance, no doubt, muſt be made for the ſmall quantities of common air, which lodged on the top of my phials, when I raiſed the fume from the ſpirit of ſalt; but, from the precautions that I made uſe of, I think that very little is to be allowed to this circumſtance; and, upon the whole, I am of opinion, that this experiment is an approach to the generation of common air, or air fit for reſpiration.

I had alſo imagined, that if air diminiſhed by the proceſſes above-mentioned was affected in this manner, in conſequence of its being ſaturated with phlogiſton, a mixture of this vapour might imbibe that phlogiſton, and render it wholeſome again; but I put about one fourth of this vapour to a quantity of air in which metals had been calcined, without making any ſenſible alteration in it. I do not, however, infer from this, that air is not diminiſhed by means of phlogiſton, ſince the air, like ſome other ſubſtances,

 may

may hold the phlogiſton too faſt, to be deprived of it by this acid vapour.

I ſhall conclude my account of theſe experiments with obſerving, that the electric ſpark is viſible in the vapour of ſpirit of ſalt, exactly as it is in common air; and though I kept making this ſpark a conſiderable time in a quantity of it, I did not perceive that any ſenſible alteration was made in it. A little inflammable air was produced, but not more than might have come from the two iron nails which I made uſe of in taking the ſparks.

X.

Miscellaneous Observations.

Many of the preceding obſervations relating to the vinous and putrefactive fermentations, I had the curioſity to endeavour to aſcertain in what manner the air would be affected by the acetous fermentation. For this purpoſe I incloſed a phial full of ſmall beer in a jar ſtanding in water, and obſerved that during the firſt two or three days there was an increaſe of the air in the jar, but from that time it gradually decreaſed, till at length there appeared to be a diminution of about $\frac{1}{10}$ of the whole quantity. During this time the whole ſurface of it was gradually covered with a ſcum, beautifully corrugated. After this there was an increaſe of the air till there was more than the original quantity; but this muſt have been fixed air, not incorporated with the reſt of the maſs; for, withdrawing the beer, which I found to be ſour, after it had ſtood 18 or 20 days under the jar, and

paſſing

paſſing the air ſeveral times through cold water, the original quantity was diminiſhed about $\frac{1}{9}$. In the remainder a candle would not burn, and a mouſe would have died preſently. The ſmell of this air was exceedingly pungent, but different from that of the putrid effluvium. A mouſe lived perfectly well in this air, thus affected with the acetous fermentation; after it had ſtood ſeveral days mixed with four times the quantity of fixed air.

All the kinds of factitious air on which I have yet made the experiment are highly noxious to animals, except that which is extracted from ſalt-petre, or alum; but in this even a candle burned juſt as in common air. In one quantity which I got from ſalt-petre a candle not only burned, but the flame was increaſed, and ſomething was heard like a hiſſing, ſimilar to the decrepitation of nitre in an open fire. This experiment was made when the air was freſh made, and while it probably contained ſome particles of nitre, which would have been depoſited afterwards. The air was extracted from theſe ſubſtances by putting them into a gun barrel, which was much corroded and ſoon ſpoiled by the experiment. What effect this circumſtance may have had upon the air I have not conſidered.

November 6, 1772, I had the curioſity to examine the ſtate of a quantity of this air, which had been extracted from ſalt-petre above a year, and which at firſt was perfectly wholeſome; when, to my very great ſurprize, I found that it was become, in the higheſt degree, noxious. It made no efferveſcence with nitrous air, and a mouſe died the moment it was put into it. I had not, however, waſhed it in rain water quite ten minutes

(and perhaps less time would have been sufficient) when I found, upon trial, that it was restored to its former perfectly wholesome state. It effervesced with nitrous air as much as the best common air ever does, and even a candle burned in it very well, which I had never before observed of any kind of noxious air meliorated by agitation in water. This series of facts, relating to air extracted from nitre, appear to me to be very extraordinary and important, and, in able hands, may lead to considerable discoveries.

There are many substances which impregnate the air in a very remarkable manner, but without making it noxious to animals. Among other things I tried volatile alkaline salts, and camphire, the latter of which I melted with a burning glass, in air inclosed in a phial. The mouse which was put into this air sneezed and coughed very much, especially after it was taken out; but it presently recovered, and did not appear to have been sensibly injured.

Having made several experiments with a mixture of iron filings and brimstone, kneaded to a paste with water, I had the curiosity to try what would be the effect of substituting brass dust in the place of the iron filings. The result was, that when this mixture had stood about three weeks, in a given quantity of air, it had turned black, but was not increased in bulk. The air also was neither sensibly increased nor decreased, but the nature of it was changed, for it extinguished flame, it would have killed a mouse presently, and was not restored by fixed air, which had been mixed with it several days.

I have

I have frequently mentioned my having, at one time, expoſed equal quantities of different kinds of air in jars ſtanding in boiled water. The common air in this experiment was diminiſhed four ſevenths, and the remainder extinguiſhed flame. This experiment demonſtrates that water does not abſorb air equally, but that it decompoſes it, taking one part, and leaving the reſt. To be quite ſure of this fact, I agitated a quantity of common air in boiled water, and when I had reduced it from eleven ounce meaſures to ſeven, I found that it extinguiſhed a candle, but a mouſe lived in it very well. At another time a candle barely went out when the air was diminiſhed one third, and at other times I have found this effect take place at other very different degrees of diminution. This difference I attribute to the differences in the ſtate of the water with reſpect to the air contained in it; for ſometimes it had ſtood longer than at other times before I made uſe of it. I alſo uſed diſtilled water, rain water, and water out of which the air had been pumped, promiſcuouſly with rain water. I even doubt not but that, in a certain ſtate of the water, there might be no ſenſible difference in the bulk of the agitated air, and yet at the end of the proceſs it would extinguiſh a candle, air being ſupplied from the water in the place of that part of the common air which had been abſorbed.

It is certainly a little extraordinary that the very ſame proceſs ſhould ſo far mend putrid air, as to reduce it to the ſtandard of air in which candles have burned out; and yet that it ſhould ſo far injure common and wholeſome air, as to reduce it to about the

the ſame ſtandard: but ſo the fact certainly is. If air extinguiſh flame in conſequence of its being previouſly ſaturated with phlogiſton, it muſt, in this caſe, have been transferred from the water to the air.

To a quantity of common air, thus diminiſhed by agitation in water, till it extinguiſhed a candle, I put a plant, but it did not ſo far reſtore it as that a candle would burn in it again; which to me appeared not a little extraordinary, as it did not ſeem to be in a worſe ſtate than air in which candles had burned out, and which had never failed to be reſtored by the ſame means. I had no better ſucceſs with a quantity of permanent air; which I had collected from my pump water. Indeed theſe experiments were begun before I was acquainted with that property of nitrous air, which makes it ſo accurate a meaſure of the goodneſs of other kinds of air; and it might perhaps be rather too late in the year when I made the experiments. Having neglected theſe two jars of air, the plants died and putrefied in both of them; and then I found the air in them both to be highly noxious, and to make no efferveſcence with nitrous air.

I found that a pint of my pump water contains about one fourth of an ounce meaſure of air, one half of which was afterwards abſorbed by ſtanding in freſh pump water. A candle would not burn in the air, but a mouſe lived in it very well. Upon the whole, it ſeemed to be in about the ſame ſtate as air in which a candle had burned out.

I once imagined that, by mere ſtagnation, air might become unfit for reſpiration, or at leaſt for the burning of candles; but if this be the caſe, and the change be produced gradually, it muſt require a long time for the purpoſe. For on the 22d of September 1772, I examined a quantity of common air, which had been kept in a phial, without agitation, from May 1771, and found it to be in no reſpect worſe than freſh air, even by the teſt of the nitrous air.

The cryſtallization of nitre makes no ſenſible alteration in the air in which the proceſs is made. For this purpoſe I diſſolved as much nitre as a quantity of hot water would contain, and let it cool under a receiver, ſtanding in water.

November 6, 1772, a quantity of inflammable air, which, by long keeping, had come to extinguiſh flame, I obſerved to ſmell very much like common air in which a mixture of iron filings and brimſtone had ſtood. It was not, however, quite ſo ſtrong, but it was equally noxious.

Biſmuth and nickel are diſſolved in the marine acid with the application of a conſiderable degree of heat; but little or no air is got from either of them; but, what I thought a little remarkable, both of them ſmelled very much like Harrowgate water. This ſmell I have met with ſeveral times in the courſe of my experiments, and in proceſſes very different from one another.

As I generally made uſe of mice in the experiments which relate to reſpiration, and ſome perſons may chuſe to repeat them after me, and purſue them farther than I have done; it may be

 of

of uſe to them to be informed, that I kept them without any difficulty in glaſs receivers, open at the top and bottom, and having a quantity of paper, or tow, in the inſide, which ſhould be changed every three or four days; when it will be moſt convenient alſo to change the veſſel, and waſh it. But they muſt be kept in a pretty exact temperature, for either much heat or much cold kills them preſently. The place in which I have generally kept them is a ſhelf over the kitchin fire place, where, as it is uſual in Yorkſhire, the fire never goes out; ſo that the heat varies very little; and I find it to be at a medium about 70 degrees of Fahrenheit's thermometer. When they had been made to paſs through the water, as they neceſſarily muſt be, in order to a change of air, they require, and will bear a very conſiderable degree of heat, to warm and dry them.

I found, to my great ſurprize, in the courſe of theſe experiments, that mice will live intirely without water; for though I have kept ſome of them for three or four months, and have offered them water ſeveral times, they would never taſte it; and yet they continued in perfect health and vigour. Two or three of them will live very peaceably together in the ſame veſſel; though I had one inſtance of one mouſe tearing another almoſt in pieces, though there was plenty of proviſions for both of them.

The apparatus with which the principal of the preceding experiments were made is exceedingly ſimple, and cheap. The drawing annexed (TAB. IX.) exhibits a view of every thing that is moſt important in it.

A is

A is an oblong trough, about eight inches deep, kept nearly full of water, and B, B are jars ſtanding in it, about ten inches long, and two and a half wide; ſuch as I have generally uſed for electrical batteries.

C, C are flat ſtones, ſunk about an inch, or half an inch, under the water, on which veſſels of any kind may be conveniently placed, during a courſe of experiments.

D, D are pots nearly full of water, in which jars or phials, containing any kind of air, to which plants or any other ſubſtances may be expoſed, and having their mouths immerſed in water; ſo that the air in the inſide can have no communication with the external air.

E is a ſmall glaſs veſſel, of a convenient ſize for putting a mouſe into it, in order to try the wholeſomeneſs of any kind of air that it may contain.

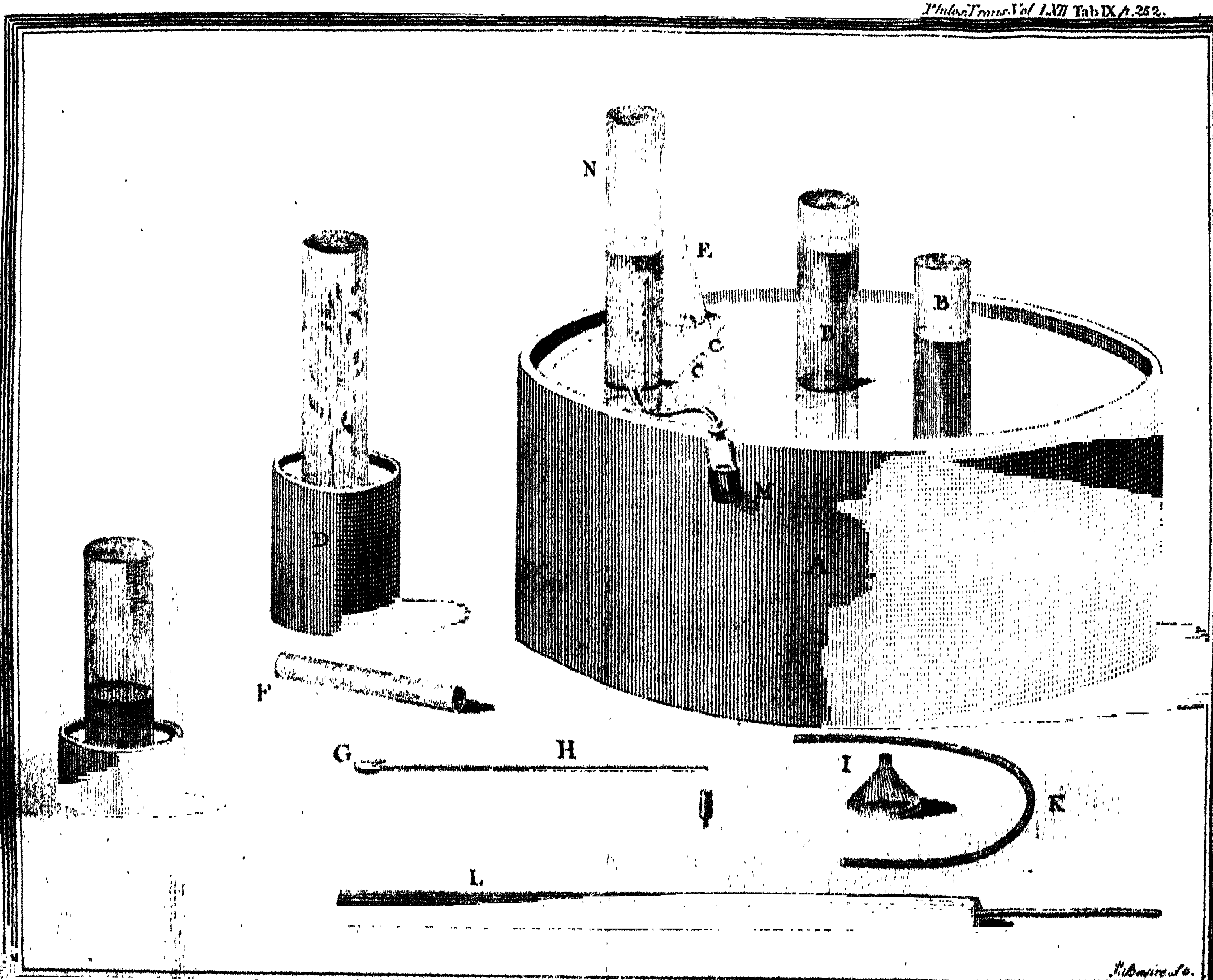
F is a cylindrical glaſs veſſel, five inches in length, and one in diameter, very proper for trying whether any kind of air will admit a candle to burn in it. For this purpoſe a bit of wax candle, G, may be faſtened to the end of a wire, H, and turned up in ſuch a manner as to be let down into the veſſel with the flame upwards. The veſſel ſhould be kept carefully covered till the moment that the candle is admitted to it. In this manner I have frequently extinguiſhed a candle above twenty times in one of theſe veſſels full of air, though it is impoſſible to dip the candle into it, without giving the external air an opportunity of mixing with it, more or leſs.

I is a funnel of glaſs or tin, which is neceſſary for transferring air into veſſels which have narrow mouths.

K is a glaſs ſyphon, which is very uſeful for drawing air out of a veſſel which has its mouth immerſed in water, and thereby raiſing the water to whatever height may be moſt convenient. I do not think it by any means ſafe to depend upon a valve at the top of a veſſel, which Dr. Hales very often made uſe of; for, ſince my firſt diſappointments, I have never thought the communication between the external and internal air ſufficiently cut off, unleſs glaſs, or a body of water, or, in ſome caſes, quickſilver, have intervened between them.

L is a piece of a gun barrel, cloſed at one end, having the ſtem of a tobacco-pipe luted to the other. To the end of this pipe I ſometimes faſtened a flaccid bladder, in order to receive the air diſcharged from the ſubſtance contained in the barrel; but, when the air was generated ſlowly, I commonly contrived to put this end of the pipe under a veſſel full of water, and ſtanding with its mouth inverted in another veſſel of water, that the new air might have a more perfect ſeparation from the external air than a bladder could make.

M is a ſmall phial containing ſome mixture that will generate air. This air paſſes through a bent glaſs tube inſerted into the cork at one end, and going under the edge of the jar N at the other; the jar being placed with part of its mouth projecting beyond the flat ſtones C C for that purpoſe.


N
E
B
B
C
C
M
D
A
F
G
H
I
K
L
J. Basire Sc.

AN APPENDIX,

Containing an account of ſome experiments made by Mr. Hey, which prove that there is no oil of vitriol in water impregnated with fixed air extracted from chalk by oil of vitriol; and alſo a letter from Mr. Hey, to Dr. Prieſtley, concerning the effects of fixed air applied by way of clyſter.

Experiments to prove that there is no oil of vitriol in water impregnated with fixed Air.

It having been ſuggeſted, that air ariſing from a fermenting mixture of chalk and oil of vitriol might carry up with it a ſmall portion of the vitriolic acid, rendered volatile by the act of fermentation; I made the following experiments, in order to diſcover whether the acidulous taſte, which water impregnated with ſuch air affords, was owing to the preſence of any acid, or only to the fixed air it had abſorbed.

Experiment I.

I mixed a tea-ſpoonful of ſyrup of violets with an ounce of diſtilled water, ſaturated with fixed air procured from chalk by means of the vitriolic acid; but neither upon the firſt mixture, nor after

 ſtanding

ſtanding 24 hours, was the colour of the ſyrup at all changed, except by its ſimple dilution.

Experiment II.

A portion of the ſame diſtilled water, unimpregnated with fixed air, was mixed with the ſyrup in the ſame proportion: not the leaſt difference in colour could be perceived betwixt this and the above mentioned mixture.

Experiment III.

One drop of oil of vitriol being mixed with a pint of the ſame diſtilled water, an ounce of this water was mixed with a tea-ſpoonful of the ſyrup. This mixture was very diſtinguiſhable in colour from the two former, having a purpliſh caſt, which the others wanted.

Experiment IV.

The diſtilled water impregnated with ſo ſmall a quantity of vitriolic acid having a more agreeable taſte than when alone, and yet manifeſting the preſence of an acid by means of the ſyrup of violets; I ſubjected it to ſome other teſts of acidity. It formed curds when agitated with ſoap, lathered with difficulty, and very imperfectly; but not the leaſt ebullition could be diſcovered upon dropping in ſpirit of ſal ammoniac, or ſolution of ſalt of tartar, though I had taken care to render the latter free from cauſticity by impregnating it with fixed air.

Ex-

EXPERIMENT V.

The diſtilled water ſaturated with fixed air neither efferveſced, nor ſhewed any clouds, when mixed with the fixed or volatile alkali.

EXPERIMENT VI.

No curd was formed by pouring this water upon an equal quantity of milk, and boiling them together.

EXPERIMENT VII.

When agitated with ſoap, this water produced curds, and lathered with ſome difficulty; but not ſo much as the diſtilled water mixed with vitriolic acid in the very ſmall proportion above mentioned. The ſame diſtilled water without any impregnation of fixed air lathered with ſoap without the leaſt previous curdling. River water, and a pleaſant pump water not remarkably hard, were compared with theſe. The former produced curds before it lathered, but not quite in ſo great a quantity as the diſtilled water impregnated with fixed air: the latter cauſed a ſtronger curd than any of the others above-mentioned.

EXPERIMENT VIII.

Apprehending that the fixed air in the diſtilled water occaſioned the coagulation, or ſeparation of the oily part of the ſoap, only by deſtroying the cauſticity of the *lixivium*, and thereby rendering the

union leſs perfect betwixt that and the tallow, and not by the preſence of any acid; I impregnated a freſh parcel of the ſame diſtilled water with fixed air, which had paſſed through half a yard of a wide barometer tube filled with ſalt of tartar; but this water cauſed the ſame curdling with ſoap as the former had done, and appeared in every reſpect to be exactly the ſame.

Experiment IX.

Diſtilled water ſaturated with fixed air formed a white cloud and precipitation, upon being mixed with a ſolution of *ſaccharum ſaturni*. I found likewiſe, that fixed air, after paſſing through the tube filled with alkaline ſalt, upon being let into a phial containing a ſolution of the metallic ſalt in diſtilled water, cauſed a perfect ſeparation of the lead, in form of a white powder; for the water, after this precipitation, ſhewed no cloudineſs upon a freſh mixture of the ſubſtances which had before rendered it opaque.

A Letter from Mr. Hey to Dr. Priestley, concerning the Effects of fixed Air applied by way of Clyster.

Leeds, Feb. 15th, 1772.

Reverend Sir,

Having lately experienced the good effects of fixed air in a putrid fever, applied in a manner, I believe, not heretofore made use of, I thought it proper to inform you of the agreeable event, as the method of applying this powerful corrector of putrefaction took its rise principally from your observations and experiments on factitious air; and now, at your request, I send the particulars of the case I mentioned to you, as far as concerns the administration of this remedy.

January 8, 1772, Mr. Lightbowne, a young gentleman who lives with me, was seized with a fever, which, after continuing about ten days, began to be attended with those symptoms that indicate a putrescent state of the fluids.

18th, His tongue was black in the morning when I first visited him, but the blackness went off in the day-time upon drinking: He had begun to doze much the preceding day, and now he took little notice of those that were about him: His belly was loose, and had been so for some days: his pulse beat 110 strokes in a minute, and was rather low: he was ordered to take twenty five grains of Peruvian bark with five of tormentill root in powder every four hours, and to use red wine and water cold as his common drink.

19th,

19th, I was called to viſit him early in the morning, on account of a bleeding at the noſe which had come on: he loſt about eight ounces of blood, which was of a looſe texture: the hæmorrhage was ſuppreſſed, though not without ſome difficulty, by means of tents made of ſoft lint, dipped in cold water ſtrongly impregnated with tincture of iron, which were introduced within the noſtrils quite through to their poſterior apertures; a method which has never yet failed me in like caſes. His tongue was now covered with a thick black pellicle, which was not diminiſhed by drinking: his teeth were furred with the ſame kind of ſordid matter, and even the roof of his mouth and fauces were not free from it: his looſeneſs and ſtupor continued, and he was almoſt inceſſantly muttering to himſelf: he took this day a ſcruple of the Peruvian bark with ten grains of tormentill every two or three hours: a ſtarch clyſter containing a drachm of the compound powder of bole, without opium, was given morning and evening: a window was ſet open in his room, though it was a ſevere froſt, and the floor was frequently ſprinkled with vinegar.

20th, He continued nearly in the ſame ſtate: when rouzed from his dozing, he generally gave a ſenſible anſwer to the queſtions aſked him; but he immediately relapſed, and repeated his muttering. His ſkin was dry, and harſh, but without *petechiæ*. He ſometimes voided his urine and *fæces* into the bed, but generally had ſenſe enough to aſk for the bed-pan: as he now nauſeated the bark in ſubſtance, it was exchanged for Huxham's

tincture,

tincture, of which he took a table-ſpoonful every two hours in a cup full of cold water: he drank ſometimes a little of the tincture of roſes, but his common liquors were red wine and water, or rice water and brandy acidulated with elixir of vitriol: before drinking, he was commonly requeſted to rinſe his mouth with water to which a little honey and vinegar had been added. His looſeneſs rather increaſed, and the ſtools were watery, black, and fœtid: It was judged neceſſary to moderate this diſcharge, which ſeemed to ſink him, by mixing a drachm of the *theriaca Andromachi* with each clyſter.

21ſt. The ſame putrid ſymptoms remained, and a *ſubſultus tendinum* came on: his ſtools were more fœtid; and ſo hot, that the nurſe aſſured me ſhe could not apply her hand to the bed-pan, immediately after they were diſcharged, without feeling pain on this account: The medicine and clyſters were repeated.

Reflecting upon the diſagreeable neceſſity we ſeemed to lie under of confining this putrid matter in the inteſtines, leſt the evacuation ſhould deſtroy the *vis vitæ* before there was time to correct its bad quality, and overcome its bad effects, by the means we were uſing; I conſidered, that, if this putrid ferment could be more immediately corrected, a ſtop would probably be put to the flux, which ſeemed to ariſe from, or at leaſt to be encreaſed by it; and the *fomes* of the diſeaſe would likewiſe be in a great meaſure removed. I thought nothing was ſo likely to effect this, as the introduction of fixed air into the alimentary canal,

 which,

which, from the experiments of Dr. Macbride, and those you have made since his publication, appears to be the most powerful corrector of putrefaction hitherto known. I recollected what you had recommended to me as deserving to be tried in putrid diseases, I mean, the injection of this kind of air by way of clyster, and judged that in the present case such a method was clearly indicated.

The next morning I mentioned my reflections to Dr. Hird and Dr. Crowther, who kindly attended this young gentleman at my request, and proposed the following method of treatment, which, with their approbation, was immediately entered upon. We first gave him five grains of ipecacoanha, to evacuate in the most easy manner part of the putrid *colluvies*: he was then allowed to drink freely of brisk orange-wine, which contained a good deal of fixed air, yet had not lost its sweetness: the tincture of bark was continued as before; and the water, which he drank along with it, was impregnated with fixed air from the atmosphere of a large vat of fermenting wort, in the manner I had learned from you: instead of the astringent, air alone was injected, collected from a fermenting mixture of chalk and oil of vitriol: he drank a bottle of orange-wine in the course of this day, but refused any other liquor except water and his medicine: two bladders full of air were thrown up in the afternoon.

23d. His stools were less frequent; their heat likewise and peculiar *fœtor* were considerably diminished: his muttering was much abated, and the *subsultus tendinum* had left him. Finding that part of the air was rejected when given with a bladder in the

the uſual way, I contrived a method of injecting it which was not ſo liable to this inconvenience. I took the flexible tube of that inſtrument which is uſed for throwing up the fume of tobacco, and tied a ſmall bladder to the end of it that is connected with the box made for receiving the tobacco, which I had previouſly taken off from the tube: I then put ſome bits of chalk into a ſix ounce phial until it was half filled; upon theſe I poured ſuch a quantity of oil of vitriol as I thought capable of ſaturating the chalk, and immediately tied the bladder, which I had fixed to the tube, round the neck of the phial: the clyſter pipe, which was faſtened to the other end of the tube, was introduced into the *anus* before the oil of vitriol was poured upon the chalk. By this method the air paſſed gradually into the inteſtines as it was generated; the rejection of it was in a great meaſure prevented; and the inconvenience of keeping the patient uncovered during the operation was avoided.

24th, He was ſo much better, that there ſeemed to be no neceſſity for repeating the clyſters: the other means were continued. The window of his room was now kept ſhut.

25th, All the ſymptoms of putreſcency had left him; his tongue and teeth were clean; there remained no unnatural blackneſs or *fœtor* in his ſtools, which had now regained their proper conſiſtence; his dozing and muttering were gone off; and the diſagreeable odour of his breath and perſpiration was no longer perceived. He took nouriſhment to-day, with pleaſure; and, in the afternoon, ſat up an hour in his chair.

His

His fever, however, did not immediately leave him; but this we attributed to his having caught cold from being incautiously uncovered, when the window was open, and the weather extremely severe; for a cough, which had troubled him in some degree from the beginning, increased, and he became likewise very hoarse for several days, his pulse, at the same time, growing quicker: but these complaints also went off, and he recovered, without any return of the bad symtoms above-mentioned.

I am, Reverend Sir,

Your obliged humble servant,

Wm Hey.

P. S.

October 29, 1772.

Fevers of the putrid kind have been so rare in this town, and in its neighbourhood, since the commencement of the present year, that I have not had an opportunity of trying again the effects of fixed air, given by way of clyster, in any case exactly similar to Mr. Lightbowne's. I have twice given water saturated with fixed air in a fever of the putrescent kind, and it agreed very well with the patients. To one of them the aërial clysters were administred, on account of a looseness, which attended the fever, though the stools were not black, nor remarkably hot or foetid.

These

These clysters did not remove the looseness, though there was often a greater interval than usual betwixt the evacuations, after the injection of them. The patient never complained of any uneasy distention of the belly from the air thrown up, which, indeed, is not to be wondered at, considering how readily this kind of air is absorbed by aqueous and other fluids, for which sufficient time was given, by the gradual manner of injecting it. Both those patients recovered, though the use of fixed air did not produce a crisis before the period on which such fevers usually terminate. They had neither of them the opportunity of drinking such wine as Mr. Lightbowne took after the use of fixed air was entered upon; and this, probably, was some disadvantage to them.

I find the methods of procuring fixed air, and impregnating water with it, which you have published, are preferable to those I made use of in Mr. Lightbowne's case.

The flexible tube used for conveying the fume of tobacco into the intestines, I find to be a very convenient instrument in this case, by the method before-mentioned (only adding water to the chalk, before the oil of vitriol is instilled, as you direct): the injection of air may be continued at pleasure, without any other inconvenience to the patient, than what may arise from his continuing in one position during the operation, which scarcely deserves to be mentioned, or from the continuance of the clyster-pipe within the anus, which is but trifling, if it be not shaken much, or pushed against the rectum.

When I said in my letter, that fixed air appeared to be the greatest corrector of putrefaction hitherto

 known,

known, your philoſophical reſearches had not then made you acquainted with that moſt remarkably antiſeptic property of nitrous air. Since you favoured me with a view of ſome aſtoniſhing proofs of this, I have conceived hopes, that this kind of air may likewiſe be applied medicinally to great advantage.

W. H.

A Correction.

Upon re-examining Dr. Hales's account of his experiments to meaſure the diminution of air by reſpiration (Statical Eſſays, Vol. I. p. 238, 4th edition), I find an error of the preſs, of [illegible] for [illegible]; ſo that the diminution of air by reſpiration, though very various, is, I believe, always conſiderably leſs than by putrefaction, or ſeveral other cauſes of diminution. But though I have mentioned this diminution as equal to ſeveral others, nothing material depends upon it: the quality of the air thus diminiſhed being, in all reſpects, the ſame, notwithſtanding the cauſe of increaſe (which, as I have obſerved, in this and other caſes, co-operates with the cauſe of diminution) be greater than I had ſuppoſed.

I did not endeavour to meaſure the quantity of the diminution of air by reſpiration, as I did that by other cauſes; becauſe I imagined that it had been done ſufficiently by others, and eſpecially by Dr. Hales.

XX. An

Received November 29, 1771.

XX. *An Essay on the periodical Appearing and Disappearing of certain Birds, at different Times of the Year. In a Letter from the Honourable* Daines Barrington, *Vice-Pres. R. S. to* William Watson, *M. D. F. R. S.*

DEAR SIR,

Read April 2, 9, 30, and May 14, 1772.

AS I know, from some conversation we have had on this head, that you consider the migration of birds as a very interesting point in natural history, I send you the following reflections on this subject as they have occurred to me upon looking into most of the ornithologists who have written on this question.

It will be first necessary in the present, as in all other disputes, to define the terms on which the controversy arises. I therefore premise that I mean by the word Migration, a periodical passage by a whole species of birds across a considerable extent of sea.

I do not mean therefore to deny that a bird, or birds, may possibly fly now and then from Dover to

 Calais,

Calais, from Gibraltar to Tangier, or any other ſuch narrow ſtrait, as the oppoſite coaſts are clearly within the bird's ken, and the paſſage is no more adventurous than acroſs a large freſh water lake.

I as little mean to deny that there may be a periodical flitting of certain birds from one part of a continent to another: the Royſton Crow, and Rock Ouzel, furniſh inſtances of ſuch a regular migration.

What I mean chiefly to contend therefore is, that it ſeems to be highly improbable, birds ſhould, at certain ſeaſons, traverſe large tracts of ſea, or rather ocean, without leaving any of the ſame ſpecies behind, but the ſick or wounded.

As this litigated point can only receive a ſatisfactory deciſion from very accurate obſervations, all preceding naturaliſts, from Ariſtotle to Ray, have ſpoken with much doubt concerning it.

Soon after the appearance of Monſ. Adanſon's voyage to Senegal, however, Mr. Collinſon firſt, in the Philoſophical Tranſactions *, and after him the moſt eminent ornithologiſts of Europe, ſeem to have conſidered this traveller's having caught four European Swallows on the 6th of October, not far from the African coaſt, as a deciſive proof, that the common ſwallows, when they diſappear in Europe, make for Africa during the winter, and return again to us in the ſpring.

It is therefore highly incumbent upon me, who profeſs that I am by no means ſatisfied with the account, given by Monſ. Adanſon of theſe European

* Part II. 1760, p. 459, & ſeq.

ſwallows,

ſwallows, to enter into a very minute diſcuſſion of what may, or may not, be inferred from his obſervation according to his own narrative.

I ſhall firſt however conſider the general arguments, from which it is ſuppoſed that birds of paſſage periodically traverſe oceans, which indeed may be almoſt reduced to this ſingle one, *viz.* we ſee certain birds in particular ſeaſons, and afterwards we ſee them not; from which data it is at once inferred, that the cauſe of their diſappearance is, that they have croſſed large tracts of ſea.

The obvious anſwer to this is, that no well-atteſted inſtances can be produced of ſuch a migration, as I ſhall endeavour to ſhew hereafter; but beſides this convincing negative proof, there are not others wanting.

Thoſe who ſend birds periodically acroſs the ſea, being preſſed with the very obvious anſwer I have before ſuggeſted, have recourſe to two ſuppoſitions, by which they would account for their not being obſerved by ſeamen during their paſſage.

The firſt is, that they riſe ſo high in the air that they become inviſible *; but unfortunately the riſing to this extraordinary height, or the falling from it, is equally deſtitute of any ocular proof, as the birds being ſeen during their paſſage.

I have indeed converſed with ſome people, who conceive they have loſt ſight of birds by their perpendicular flight; I muſt own, however, that I have

* It is well known that ſome ornithologiſts have even ſuppoſed that they leave our atmoſphere for that of the Moon. See Harl. Miſc. Vol. II. p. 561.

always ſuppoſed them to be ſhort-ſighted, as I never loſt the ſight of a bird myſelf, but from its horizontal diſtance, and I doubt much whether any bird was ever ſeen to riſe to a greater height than perhaps twice that of St. Paul's croſs *.

There ſeems to be but one method indeed, by which the height of a bird in the air may be eſtimated; which is, by comparing its apparent ſize with its known one, when very near us; and it need not be ſaid that method of calculating muſt depend entirely upon the ſight of the obſerver, who, if he happens not to ſee objects well at a diſtance, will very ſoon ſuppoſe the bird to be loſt in the clouds.

There is alſo another objection to the hypotheſis of birds paſſing ſeas at ſuch an extraordinary height, ariſing from the known rarefaction of the air, which may poſſibly be inconvenient for reſpiration, as well as flight; and if this was not really the caſe, one ſhould ſuppoſe that birds would frequently riſe to ſuch uncommon elevations, when they had no occaſion to traverſe oceans.

* Wild geeſe fly at the greateſt height of any bird I ever happened to attend to; and from comparing them with rooks, which I have frequently looked at, when perched on the croſs of St. Paul's, I cannot think that a wild-gooſe was ever diminiſhed, to my ſight at leaſt, more than he would be at twice the height of St. Paul's, or perhaps 300 yards. Mr. Hunter, F. R. S. informs me, that the bird which hath appeared to him as the higheſt flier, is a ſmall eagle on the confines of Spain and Portugal, which frequents high rocks. Mr. Hunter hath firſt ſeen this ſpecies of eagle from the bottom of a mountain, and followed it to the top, when the bird hath riſen ſo high as to appear leſs than he did from the bottom. Mr. Hunter however adds, that he could ſtill hear the cry, and diſtinguiſh the bird.

The

The Scotch Ptarmigan frequents the higheſt ground of any Britiſh bird, and he takes but very ſhort flights.

But it is alſo urged by ſome, that the reaſon why ſeamen do not regularly ſee the migration of birds, is becauſe they chooſe the night, and not the day, for the paſſage *.

Now though it may be allowed, that poſſibly birds may croſs from the coaſt of Holland to the Eaſtern coaſt of England (for example) during a long night, yet it muſt be dark nearly as long as it is within the Arctic circle to afford time for a bird to paſs from the Line to many parts of Europe, which Monſ. de Buffon calculates, may be done in about eight or nine days †.

If the paſſage happened in half the nights of the year, which have the benefit of moonlight, the birds would be diſcovered by the ſailors almoſt as well as in the day time; to which I muſt add that ſeveral ſuppoſed birds of paſſage (the Fieldfare in particular) always call when on their flight, ſo that the ſeamen muſt be deaf as well as blind, if ſuch flocks of birds eſcape their notice.

Other objections however remain to this hypotheſis of a paſſage during the night.

* Mr. Cateſby ſuppoſes that they may thus paſs in the night-time, to avoid birds of prey. Phil. Tranſ. Abr. Vol. II. p. 887. But are not owls then ſtirring?

On the other hand, if they migrate in the day time, kites, hawks, and other birds of prey, muſt be very bad ſportſmen not to attend (like Arabs) theſe large and periodical caravans.

† In the preface to the firſt volume of his lately publiſhed Ornithology, p. 32.

Ninety-

Most birds not only sleep during the night, but are as much incapacitated from distinguishing objects well as we are, in the absence of the sun: it is therefore inconceivable that they should choose owl-light for such a distant journey.

Besides this, the Eastern coast of England, to which birds of passage must necessarily first come from the continent, hath many light-houses upon it; they would therefore, in a dark night, immediately make for such an object, and destroy themselves by flying with violence against it, as is well known to every bat-fowler.

Having endeavoured to answer these two suppositions, by which it is contended that birds of passage may escape observation in their flight; I shall now consider all the instances I have been able to meet with of any birds being actually seen whilst they were crossing any extent of sea, though I might give a very short refutation to them, by insisting, that if this was ever experienced, it must happen as constantly in a sea, which is much navigated, as the return of the seasons.

I cannot do better than to follow these according to chronological order.

The first in point of time is that which is cited by Willoughby *, from Bellon, whose words are thus transluted, " When we failed from Rhodes to " Alexandria, many quails flying from the North " towards the South, were taken in our ship, whence " I am persuaded that they shift places; for formerly, when I sailed out of the Isle of Zant to " Morea, or Negropont, in the spring, I had ob-

* B. II. c. 11. §. 8.

" served

"ſerved quails flying the contrary way to N. and S.
"that they might abide there all ſummer, at which
"time alſo a great many were taken in the ſhip."

Let us now conſider what is to be inferred from this citation.

In the firſt place, Bellon does not particularize the longitude and latitude of that part of the Mediterranean, which he was then croſſing; and in his courſe from Rhodes to Alexandria, both the iſlands of Scarpanto and Crete could be at no great diſtance: theſe quails therefore were probably flitting from one iſland of the Mediteranean * to another.

The ſame obſervation may be made with regard to the quails which he ſaw between Zant and Negropont, as the whole paſſage is crouded with iſlands, they therefore might be paſſing from iſland to iſland, or headland to headland, which might very probably lye Eaſt and Weſt, ſo as to occaſion the birds flying in a different direction, from which they paſſed the ſhip before.

I have therefore no objection to this proof of migration, if it is only inſiſted upon to ſhew that a quail ſhifts its ſtation at certain ſeaſons of the year; but cannot admit that it is fair from hence to argue that theſe birds periodically croſs large tracts of ſea.

Bellon himſelf ſtates, that when the birds ſettled upon the ſhip, they were taken by the firſt perſon who choſe to catch them, and therefore they muſt have been unequal to the ſhort flight which they were attempting.

* One of the Mediterranean iſlands is ſuppoſed to have obtained its ancient name of Ortygia from the numbers of quails.

It is very true that quails have been often pitched upon as instances of birds that migrate across seas, because they are scarcely ever seen in winter: it is well known, however, to every sportsman, that this bird never flies 300 yards at a time, and the tail being so short, it is highly improbable they should be equal to a passage of any length.

We find therefore, that quails, which are commonly supposed to leave our island in the winter, in reality retire to the sea coasts, and pick up their food amongst the sea weeds *.

I have happened lately to see a specimen of a particular species of quail, which is described by Dr. Shaw †, and is distinguished from the other kinds by wanting the hind-claw.

Dr. Shaw also states that it is a bird of passage. Now if quails really migrate from the coast of Barbary to Italy, as is commonly supposed, whence can it have arisen that this remarkable species hath escaped the notice of Aldrovandus, Olina, and the other Italian ornithologists?

When I had just finished what I have here said with regard to the migration of quails, I have had an opportunity of seeing the second volume of Monf. de Buffon's ornithology ‡; where, under this article, he contends that this bird leaves Europe in the winter.

It is incumbent upon me, therefore, either to own I am convinced by what this most ingenious and able naturalist hath urged, or to give my reasons why I

* See Br. Zool. Vol. II. p. 210. 2d Ed. octavo.
† Phys. Obs. on the kingdom of Algiers, ch. 2.
‡ See p. 459, & seq.

still

ſtill continue to diſſent from the opinion he maintains.

Though M. de Buffon hath diſcuſſed this point very much at large, yet I find only the following facts or arguments to be new.

He firſt cites the Memoirs of the Academy of Sciences *, for an account given by M. Godeheu of quails coming to the iſland of Malta in the month of May, and leaving it in September.

The firſt anſwer to this obſervation is, that the iſland of Malta is not only near to the coaſt of Africa, but to ſeveral of the Mediterranean iſlands; it therefore amounts to no more than the flitting I have before taken notice of †.

Monſ. de Buffon ſuppoſes that a quail only quits one latitude for another, in order to meet with a perpetual crop on the ground.

Now can it be ſuppoſed that there is that difference between the harveſt on the coaſt of Africa, and that of the ſmall quantity of grain which grows on the rocky iſland of Malta, that it becomes inconvenient to the bird to ſtay in Africa as ſoon as May ſets in; and neceſſary, on the other hand, to continue in Malta from May till September.

Monſ. de Buffon then ſuppoſes that quails make their paſſage in the night, as well as conceives them to be of a remarkably warm temperature ‡, and ſays

* Tom. III. p. 91 and 92.

† Both Monſ. de Godeheu and M. de Buffon ſeem to conceive that the quail ſhould fly in the ſame direction as the wind blows; but birds on the wing from point to point, which are at a conſiderable diſtance, fly againſt the wind, as their plumage is otherwiſe ruffled.

‡ As this is given for a reaſon why the African quails migrate Northward: Q. what is to become of the Icelandic quails during the ſummer?

that "*chaud comme une caille*," is in every one's mouth *.

Now in the first place their migration during the night, is contrary to Belon's account, which M. de Buffon so much relies upon, who expressly says, that the birds were caught in the day time †.

In the next place, I apprehend that "*chaud comme une caille*," alludes to the very remarkable salaciousness of this bird, and not to the constant heat of its body.

Monf. de Buffon then observes, that if quails are kept in a cage, they are remarkably impatient of confinement in the autumn and spring, whence he infers that they then want to migrate ‡; he also adds, in the same period, that this uneasiness begins an hour before the sun rises, and that it continues all the night.

This great naturalist does not state this observation as having been made by himself, and it seems upon the face of it to be a very extraordinary one.

* All birds indeed are warmer by four degrees than other animals. See some ingenious thermometrical experiments by M. Martin of Aberdeen, Edinb. 1771, 12mo.

† Upon looking a second time into Belon, he does not indeed state whether it was in the day or the night; but if it had happened in the latter, this traveller and ornithologist could not well have omitted such a circumstance. Besides this, he mentions in what direction the quails were flying, which he could not have discerned in the night.

‡ *It may also arise from this bird's being of so quarrelsome a disposition, and consequently most likely to fight with its fellow prisoners when they are all in greatest vigour after moulting, and on the return of the spring.*

M. de Buffon allows that they will fight for a grain of millet, and adds, "car parmi les animaux il faut un sujet reel pour se "battre." M. de Buffon hath never been in a cockpit.

No

No one (at leaſt with us) ever keeps quails in a cage except the poulterers, who always ſell them as faſt as they are fat, and conſequently can give no account of what happens to them during ſo long an impriſonment as this obſervation neceſſarily implies.

No ſuch remarkable uneaſineſs hath ever been attended to in any other ſuppoſed bird of paſſage during its confinement; but, allowing the fact to be as M. de Buffon ſtates, he himſelf ſupplies us with the real cauſe of this impatience.

He aſſerts, that quails conſtantly moult twice * a year, *viz.* at the cloſe both of ſummer and winter; whence it follows, that the bird, in autumn and the ſpring, muſt be in full vigour upon its recovery from this periodical illneſs: it can therefore as little brook confinement, as the phyſician's patient upon the return of health after illneſs.

Thus much I have thought it neceſſary to ſay, in anſwer to M. de Buffon, who "dum errat, docet," who ſcarcely ever argues ill but when he is miſinformed as to facts, and who often, from ſtrength of underſtanding, diſbelieves ſuch intelligence as might impoſe upon a naturaliſt of leſs acuteneſs and penetration.

* I have often heard that certain birds moult twice a year, ſome of which I have kept myſelf without their changing their feathers more than once.

I ſhould ſuppoſe that this notion ariſes from ſome birds not moulting regularly in the autumn every year; and when the change takes place in the following ſpring, they very commonly die: I can ſcarcely think that many of them are equal to two illneſſes of ſo long a continuance, which are conſtantly to return within twelvemonths.

I ſhould therefore rather account for the extraordinary briſkneſs of a quail in autumn and the ſpring, from its recovery after moulting in the former, and from the known effects of the ſpring as to moſt animals in the latter.

The next inſtance of a bird being caught at any diſtance from land, is in Sir Hans Sloane's voyage to Jamaica, who ſays, that a lark was taken in the ſhip 40 leagues from the ſhore: this therefore was certainly an unfortunate bird, forced out to ſea by a ſtrong wind in flying from headland to headland, as no one ſuppoſes the ſkylark to be a bird of paſſage.

The ſame anſwer may be given to a yellow-hammer's ſettling upon Haſſelquiſt's ſhip in the entrance of the Mediterranean, with this difference, that either the European or African coaſt muſt have been much nearer than 40 leagues *.

The next fact to be conſidered is what is mentioned in a letter of Mr. Peter Collinſon's, printed in the Philoſophical Tranſactions †.

He there ſays, " That Sir Charles Wager had " frequently informed him, that in one of his " voyages home in the ſpring as he came into ſoundings in our channel, that a great flock of ſwallows " almoſt covered his rigging, that they were nearly " ſpent and famiſhed, and were only feathers and " bones; but being recruited by a night's reſt, they " took their flight in the morning."

The firſt anſwer to this is, that if theſe were birds which had croſſed large tracts of ſea in their periodical migrations, the ſame accident muſt happen eternally, both in the ſpring and autumn, which is not however pretended by any one.

In the next place, the ſwallows are ſtated to be ſpent both by famine and fatigue; and how were they to procure any flies or other ſuſtenance on the

* See Haſſelquiſt's Travels, in princ.
† 1760. Part II. p. 461.

rigging of the admiral's ſhip, though they migth indeed reſt themſelves?

Sir Charles, however, expreſly informs us, that he was in the channel, and within ſoundings: theſe birds, therefore (like Bellon's quails) were only paſſing probably from headland to headland; and being forced out by a ſtrong wind, were obliged to ſettle upon the firſt ſhip they ſaw, or otherwiſe muſt have dropped into the ſea, which I make no doubt happens to many unfortunate birds under the ſame circumſtances.

As the birds which thus ſettled upon Sir Charles Wager's rigging were ſwallows, it very naturally brings me now to conſider the celebrated obſervation of Monſ. Adanſon, under all its circumſtances, as it hath been ſo much relied upon, and by naturaliſts of ſo great eminence.

Monſ. Adanſon is a very ingenious writer, and the publick is much indebted to him for many of the remarks which he made whilſt he reſided in Senegal.

I may, however, I think, preſume to ſay, that he had not before his voyage made ornithology his particular ſtudy; proofs of which are not wanting in other parts of his work, which do not relate to ſwallows.

For example, he ſuppoſes, that the Canary birds which are bred in Europe are white, and that they become ſo by our climate's being more cold than that of Africa.

"J'ai remarqué que le ſerin qui devient tout blanc
"en France, eſt a Teneriffe d'un gris preſque auſſi
"foncé que celui de la linotte; ce changement de
"couleur provient vraiſemblablement de la froidure
"de notre climat *."

* Voyage au Senegal, p. 13.

Mr.

Mr. Adanſon in this paſſage ſeems to have deduced two falſe inferences from having ſeen a few white Canary birds in France, which he afterwards compares with thoſe of Teneriff, and ſuppoſes the change of colour to ariſe merely from alteration of climate: it is known, however, almoſt to every one, that there is an infinite variety in the plumage of the European Canary birds, which, as in poultry, ariſes from their being pampered with ſo much food, as well as confinement *.

Monſ. Adanſon, in another part of his voyage †, deſcribes a Roller, which he ſuppoſes to migrate ſometimes to the Southern parts of Europe.

This circumſtance ſhews that he could not have looked much into books of natural hiſtory, becauſe the principal ſynonym of this bird is *garrulus Argentoratenſis* ‡; and Linnæus informs us that it is found even in Sweden ||.

* In the ſame paſſage, he compares the colour of the African Canary bird to that of the European linnet, and ſays it is *d'un gris preſque auſſi foncé*, whereas the European linnet is well known to be brown, and not grey. The linnet affords a very deciſive proof that the change of plumage does not ariſe from the difference of climate, but the two cauſes I have aſſigned. The cock bird, whilſt at liberty, hath a red breaſt: yet if it is either bred up in a cage from the neſt, or is caught with its red plumage, and afterwards moults in the houſe, it never recovers the red feathers.

That moſt able naturaliſt, Monſ. de Buffon, from having ſeen ſome cock linnets which had thus moulted off, or perhaps ſome hen linnets (which have not a red breaſt) conſiders them as a diſtinct ſpecies, and compares their breeding together in an aviary, to that of the Canary bird and goldfinch. Ornith. p. xxii.

† P. 16. ‡ Or of Straſburgh.

|| Faun. Suec. 94.

The

The ſtrong characteriſtic mark of this bird, is the outermoſt feathers of the tail, which able naturaliſts deſcribe as three fourths of an inch longer than the reſt *. Monſ. Adanſon, however, compares their length, not with the other feathers of the tail, but with the length of the bird's body, which is by no means the natural or proper ſtandard of compariſon.

The reaſon of my taking notice of theſe more minute inaccuracies in Monſ. Adanſon's account of birds, ariſes from Mr. Collinſon's relying upon his obſervations with regard to ſwallows being ſo abſolutely deciſive, becauſe he is repreſented to be ſo able a naturaliſt.

I ſhall now ſtate (very minutely) under what circumſtances theſe ſwallows were caught, and what ſeems to be the true inference from his own account.

He informs us, that four ſwallows ſettled upon the ſhip, not 50 leagues from the coaſt of Senegal, on the 6th of October; that theſe birds were taken, and that he knew them to be the true ſwallow of Europe †, which he ſuppoſes were then returning to the coaſt of Africa.

I ſhall now endeavour to ſhew that theſe birds could not be European ſwallows; nor, if they were, could they have been on their return from Europe to Africa.

* Willoughby, p. 131. Br. Zool. Vol. II. in append.

† I have before endeavoured to ſhew that Monſ. Adanſon does not always recollect with accuracy the plumage of the moſt common European birds, by what he ſays with regard to the linnet.

The

The word *hirondelle*, in French, is used as a general term for the four * species of these birds, as the term *swallow* is with us.

Now the four swallows thus caught and examined by Monf. Adanson were either all of the same species, or intermixed in some other proportion.

Would not then any naturalist in stating so material a fact (as he himself supposes it to be) have particularized of what species of swallow these very interesting birds were?

Should not Monf. Adanson also have taken care to distinguish these supposed European swallows from two species of the same tribe, which bear a general resemblance to those of Europe, and are not only described, but engraved by Brisson, under the name of *Hirondelle de Senegal* & *Hirondelle de rivage du Senegal* †?

Though Monf. Adanson was above a year on this part of the African coast, paid so much attention to swallows, and was so immediately acquainted with the different species on the first inspection, yet he seems never to have discovered that there were such African swallows as are thus described and engraved by Brisson, though he must have seen them daily.

Monf. Adanson however concludes his account of the supposed European swallow, whilst it continues on the coast of Senegal, by a circumstance which

* *Viz.* the swallow κατ' ἐξοχήν, the martin, the sand martin, and the swift: I omit the goatsucker, because this bird, though properly classed as a species of swallow by ornithologists, is not so considered by others.

† See Brisson, Tom. II. pl. xiv.

seems

ſeems to prove to demonſtration of what ſpecies the four ſwallows caught in the ſhip really were.

He ſays that they rooſt on the ſand either by themſelves, or at moſt only in pairs, and that they frequent the coaſt much more than the inland parts *.

Theſe ſwallows therefore, if they came from Europe, muſt have immediately changed at once their known habits: and is it not conſequently moſt clear that they were of that ſpecies which Briſſon deſcribes under the name of *Hirondelle de rivage du Senegal?*

But though it ſhould be admitted, notwithſtanding what I have inſiſted upon, from Monſ. Adanſon's own account, that theſe were really ſwallows of the ſame kind with thoſe of Europe; yet I muſt ſtill contend that they could not poſſibly have been on their return from Europe to Africa, becauſe the high road for a bird from the moſt Weſtern point of Europe to Senegal, is along the N. Weſt coaſt of Africa, which projects greatly to the Weſtward of any part of Europe.

What then could be the inducement to theſe four ſwallows to fly 50 leagues to the Weſtward of the coaſt of Senegal, ſo much out of the proper direction?

It ſeems to me therefore, very clear, that theſe ſwallows (whether of the European kind or not) were flitting from the cape de Verde iſlands to the

* Voyage au Senegal, p. 67. I wiſh Monſ. Adanſon had alſo informed us whether theſe ſwallows had the ſame notes with thoſe of Europe, which is a very material circumſtance in the natural hiſtory of birds, though little attended to by moſt ornithologiſts.

 coaſt

coaſt of Africa, to which ſhort flight, however, they were unequal, and were obliged from fatigue to fall into the ſailors hands.

Monſ. Adanſon likewiſe mentions * that the ſhip's company caught a Roller on the 26th of April, which he ſuppoſes was on its paſſage to Europe, though he was then within ſight of the coaſt of Senegal: this bird, however, muſt be admitted not to have had ſufficient ſtrength to reach the firſt ſtage of this round-about journey, and was therefore probably forced out to ſea by a ſtrong wind, in paſſing from head-land to head-land.

But I muſt not diſmiſs what hath been obſerved with regard to the ſwallows ſeen by Monſ. Adanſon at Senegal, without endeavouring alſo to anſwer what M. de Buffon hath not only inferred from it, but hath endeavoured to confirm by an actual experiment †.

M. de Buffon, from the many inſtances of ſwallows being found torpid even under water, very readily admits, that all the birds of this genus do not migrate, but only that ſpecies which was ſeen by Monſ. Adanſon in Africa, and which he generally refers to as the chimney ſwallow ‡; but from the outſet, ſeems

* Voyage au Senegal, p. 15.

† See the two prefatory diſcourſes to his ſixteenth volume of natural hiſtory.

‡ So little do naturaliſts know of this very common bird, that I believe it hath never yet been obſerved by any writer, that the male ſwallow hath only the long ſlender feathers in the tail, which are conſidered as its moſt diſtinguiſhing marks. I venture to make this remark upon having ſeen the difference in two ſwallows which are in Mr. Tunſtall's collection, F. R. S. as alſo in two others, which have lately been preſented to the Muſeum

to ſhew that he hath himſelf confounded this ſpecies with the martin.

" Prenons un ſeul oiſeau, par exemple, l'hiron-
" delle, celle que tout le monde connoit, qui paroit
" au printems, diſparoit en automne, & fait ſon nid
" avec de la terre contre les fenetres, ou dans les
" cheminees." p. 23.

It is very clear that the deſign in this period is to ſpecify a particular bird in ſuch a manner that no doubt could remain with any one about the ſpecies referred to; and from other paſſages which follow, it is as clear that Monſ. de Buffon means to allude to the ſwallow κατ' εξοχην.

Though this was certainly the intention of this moſt ingenious naturaliſt, it is to me very evident that the martin, and not the ſwallow, was in his contemplation, becauſe he firſt ſpeaks of the bird's building againſt windows, before he mentions chimneys, and therefore ſuppoſes that either place is indifferent; which is not the caſe, becauſe the ſwallow ſeldom builds on the ſides of windows, or the martin in chimneys.

There are perhaps three or four martins to one ſwallow in all parts; and from their being the more common bird of the two, as well as from the circumſtance of their building at the corner of windows (and conſequently being eternally in our ſight), nine-

of the Royal Society, by the directors of the Hudſon's Bay company.

These long feathers would be very inconvenient to the hen during incubation; and they are likewiſe confined to the cock *widow-bird*, as, from their more extraordinary length, they would be ſtill more ſo.

teen out of twenty, when they speak of a swallow, really mean a martin *.

I only take notice of this supposed inaccuracy in Monf. de Buffon, because, if that able naturalist does not speak of the different sorts of swallows with that precision which is necessary upon such an occasion, why should he rely so intirely upon the impossibility of Monf. Adanson's being mistaken?

I shall now state the experiment of Monf. de Buffon, to prove that the swallow is not torpid in the winter, and must therefore migrate to the coast of Senegal †.

He shut up some swallows (*hirondelles*) in an ice house, which were there confined "plus ou moins "de temps;" and the consequence was, that those which remained there the longest died, nor could they be revived by exposing them to the sun; and, that those "qui n'avoient souffert le froid de la "glaciere que pendant peu de tems" were very lively when permitted to make their escape.

* In the same manner the generical name in other languages, for this tribe of birds, always means the martin, and not the swallow.

Thus Anacreon complains of the χελιδων for waking him by its twittering.

Now if it be considered that there was only the kitchen chimney in a Grecian house, it must have been the martin which built under the eaves of the window, that was troublesome to Anacreon, and not the swallow.

Ovid also speaking of the nest of the *hirundo*, says,

——luteum sub trabe figit opus.

by which he necessarily alludes to the martin, and not the swallow.

† Plan de l'ouvrage, p. 15.

Monf.

Monſ. de Buffon does not, in this account of his experiment, ſtate the time during which the birds were confined; but as the trial muſt have been made in France, the ſwallows which he procured could not be expected to be torpid either in an ice-houſe * or any other place, becauſe the ſeaſon for their being in that ſtate was not yet arrived.

I cannot alſo agree with M. de Buffon that thoſe birds which were ſhut up the longeſt time died through cold, as he ſuppoſes, but for want of food, as he neither ſupplied them with any flies, nor, if he had, could the ſwallows have caught them in the dark: a very ſhort faſt kills theſe tender animals, which are feeding every inſtant when on the wing.

It therefore ſeems not to follow from this, or any other experiment, that ſwallows muſt neceſſarily migrate (as Monſ. de Buffon ſuppoſes) to the coaſt of Senegal.

* The very name of an ice-houſe almoſt ſtrikes one with a chill; I placed, however, a thermometer in one near Hyde Park Corner, on the 23d of November, where it continued 48 hours, and the mercury then ſtood at 43½ by Fahrenheit's ſcale.

This is therefore a degree of cold which ſwallows ſometimes experience whilſt they continue in ſome parts of Europe, without any apparent inconvenience; and it ſhould ſeem that the cold vapours which may ariſe from the included ice, ſink the thermometer only 7 or 8 degrees, as the temperature in approved cellars is commonly from 50 or 51 throughout the year.

Sir William Hamilton informs me, that he hath frequently ſeen ſwallows in the winter between Naples and Puzzuoli, when the weather was warm; as does Mr. Hunter, F. R. S. that he hath obſerved them during the ſame ſeaſon, on the confines of Spain and Portugal. It ſhould ſeem from this, that very mild and warm weather for any continuance always wakes theſe birds from their ſtate of torpidity.

Swallows are ſeen during the ſummer, in every part of Europe from Lapland to the Southern coaſt of Spain; nor is Europe vaſtly inferior in point of ſize to Africa.

If ſwallows therefore retreat to Africa in the winter, ſhould not they be diſperſed over the whole Continent of Africa, juſt as they are over every part of Europe?

But this moſt certainly is not ſo: Dr. Shaw, who was a very good naturaliſt and attended much to the birds in the neighbourhood of Algiers (as appears by his account of that country), makes no mention of any ſuch circumſtance, nor have we heard of it from any other traveller *.

It muſt be admitted indeed, that Herodotus ſpeaking of a part of upper Egypt (which he had never ſeen) ſays, that kites and ſwallows never leave it †; this, however, totally differs from Monſ. Adanſon's account, who informs us that they diſappear in Senegal on the approach of ſummer.

It ſeems to follow therefore, from this ſilence in others, that ſwallows cannot be accommodated for their winter reſidence in any part of that vaſt continent, but in the neighbourhood of Senegal.

But this is not the whole objection to ſuch an hypotheſis.

* It may alſo be obſerved here, that credit is in ſome meaſure given to M. Adanſon's eyeſight, againſt that of all the Engliſh, French, Dutch, Portugueze, and Danes, who have been ſettled not far from Senegal for above a century, many of which have ſpent the greateſt part of their lives there, and whoſe notice, ſwallows ſeen during the winter, muſt have probably attracted.

† Ικτινοι δε και χελιδονες δι ετεος εοντες ουκ απολειπουσι. Euterpe, p. 98. ed. Gale.

If

If the ſwallows of Europe, when they diſappear in thoſe parts, retreat to the coaſt of Senegal, what neceſſarily follows with regard to a Lapland ſwallow?

I will ſuppoſe ſuch a bird to have arrived ſafely at his winter quarters upon the approach of that ſeaſon in Lapland; but he muſt then, according both to Monſ. Adanſon's and de Buffon's account, return to Lapland in the ſpring, or at leaſt ſome other ſwallow from Senegal fill his place*.

Such a bird immediately upon its arrival on the Southern coaſt of Spain would find the climate and food which it deſired to attain, and all proper conveniences for its neſt: what then is to be its inducement for quitting all theſe accommodations which it meets with in ſuch profuſion, and puſhing on immediately over ſo many degrees of European continent to Lapland, where both martin and ſwallow can procure ſo few eaves of houſes to build upon? What alſo is to be the inducement to theſe birds, when they have arrived at that part of the Norwegian coaſt which is oppoſite to the Ferroe iſlands, to croſs degrees of ſea, in order

* Mr. Stephens, A. S. S. informs me, that there was a neſt of martins for twenty years together in the hall of his houſe in Somerſetſhire (near Bath); nor could the old birds procure food either for themſelves, or their young, till the door was opened in the morning.

Can it it be ſuppoſed that the ſame birds or their deſcendants could have ſo long fixed upon ſo very inconvenient a ſpot, to which they conſtantly returned from the coaſt of Africa, neglecting ſo many others, which they muſt have always paſſed by? Does it not alſo afford a moſt ſtrong preſumption, that they were torpid during winter in the neighbourhood of this old hall?

to

to build in ſuch ſmall ſpots of land, where there are ſtill fewer houſes?

The next fact I have happened to meet with of a bird's being ſeen at a conſiderable diſtance from the ſhore, is in Mr. Forſter's lately publiſhed tranſlation of Kalm's account of N. America*.

We are there informed that a bird (which Kalm calls a ſwallow) was ſeen near the ſhip on the 2d of September, and, as he ſuppoſes, 20 degrees from the continent of America †.

It appears however, by what he before ſtates in his journal, that the ſhip was not above 5 degrees from the iſland of Sable.

Beſides, if it is contended that this was an European ſwallow on its paſſage acroſs the Atlantic on the 2d of September, it is too early even for a ſwift, to have been on its migration, which diſappears with us ſooner than the three other ſpecies of European ſwallows ‡.

Only two more inſtances have occurred of birds being ſeen in *open* ſea that have been deſcribed

* Vol. I. p. 24.

† It may not be improper here to obſerve, that in all inſtances of birds being ſeen at ſea any great diſtance from the coaſt, it is not improbable that they may have before ſettled on ſome other veſſel, or perhaps on a piece of floating wreck.

By accidents of this ſort, even butterflies have ſometimes been caught by the ſailors at 40 leagues diſtance from any land. See Monſ. l'Abbé Courte de la Blanchadiere's Voyage to Brazil, Paris, 1759, 21mo. p. 169.

‡ The bird mentioned by Kalm was probably an American ſwallow, forced out to ſea by ſome accidental ſtorm: there are ſeveral ſpecies of them and they ſeem to bear a general affinity to thoſe of Europe.

with

with any ſort of preciſion, which I ſhall juſt ſtate, as I would not decline giving the beſt anſwer I am able to every argument and fact which may be relied upon, by thoſe who contend that birds periodically migrate acroſs oceans.

On the 30th of March, 1751, Oſbeck, in his voyage from Sweden to China *, met with a ſingle houſe ſwallow near the Canary Iſlands, which was ſo tired that it was caught by the ſailors: Oſbeck alſo ſtates, that though it had been fine weather for ſeveral preceding days, the bird was as wet as if it had juſt emerged from the bottom of the ſea.

If this inſtance proves any thing, it is the ſubmerſion and not the migration of ſwallows ſo generally believed in all the northern parts of Europe. It would ſwell this Letter to a moſt unreaſonable ſize, to touch only upon this litigated point; and I ſhall, for the preſent, ſuppreſs what hath happened to occur to me on this controverted queſtion †.

* See the lately publiſhed tranſlation of this voyage.

† I will, however, mention one moſt deciſive fact on this head.

Mr. Stephens, A. S. S. informs me, that, when he was fourteen years of age, a pond of his father's (who was vicar of Shrivenham in Berkſhire) was cleaned, during the month of February; that he picked up himſelf a cluſter of three or four ſwallows (or martins), which were caked together in the mud, and that he carried them into the kitchen, on which they ſoon afterwards flew about the room, in the preſence of his father, mother, and others. Mr. Stephens alſo told me, that his father (who was a naturaliſt) obſerved at the time, he had read of ſimilar inſtances in the northern writers. This fact is alſo confirmed to me by the Reverend Dr. Pye, who was then at ſchool in Shrivenham, as alſo by a very ſenſible land-ſurveyor, who now lives in the village.

Ofbeck afterwards, in the courfe of his voyage, mentions, that a fwallow (indefinitely) followed the fhip, near Java, on the 24th of July, and another on the 14th of Auguft, in the Chinefe fea, as he terms it.

After what I have obferved before with regard to other inftances of the fame fort, I need fcarcely fay that this naturalift does not ftate of what fpecies thefe fwallows were; and that, from the latitudes in which they were feen, they muft have been fome of the Afiatic kinds.

I cannot, however, difmifs this article of the fwallow, without adding fome general reafons, which feem to prove the great improbability of this or any other bird's periodically migrating over wide tracts of fea; and I the rather do it in this place, becaufe

There are feveral reafons why fwallows fhould not be frequently thus found; ponds are feldom cleaned in the winter, as it is fuch cold work for the labourers; and the fame inftinct which prompts the bird thus to conceal itfelf, inftructs it to choofe fuch a place of fecurity, that common accidents will not difcover it.

But the ftrongeft reafon for fuch accounts not being more numerous, is, that facts of this fort are fo little attended to; for though I was born within half a mile of this pond, and have always had much curiofity with regard to fuch facts, yet I never heard a fyllable about this very material and interefting account, till very lately.

To this fact I muft alfo add, that fwallows may be conftantly taken in the month of October, during the dark nights, whilft they fit on the willows in the Thames, and that one may almoft inftantaneoufly fill a large fack with them, becaufe at this time they will not ftir from the twigs, when you lay your hands upon them. This looks very much like their beginning to be torpid before they hide themfelves under the water.

A man near Brentford fays, that he hath caught them in this ftate in the eyt oppofite to that town, even fo late as November.

the

the ſwallow is commonly pitched upon as the moſt notorious inſtance of ſuch a regular paſſage.

This ſeems to ariſe firſt from its being ſeen in ſuch numbers during the ſummer, from its appearing almoſt always on the wing, and from its feeding in that poſition; from which two latter circumſtances it is ſuppoſed to be the beſt adapted for ſuch diſtant migrations.

And firſt, let us conſider, from the few facts or reaſons we have to argue from, what length of flight either a ſwallow or any other bird is probably equal to.

A ſwallow, it is true, ſeems to be always on the wing; but I have frequently attended, as much as I could, on a particular one; and it hath appeared to me, that the bird commonly returned to its neſt in eight or ten minutes: as for extent of flight, I believe I may venture to ſay, that theſe birds are ſeldom a quarter of mile from their mate or young ones; they feed whilſt on the wing, and are perpetually turning ſhort round to catch the inſects, who endeavour to elude them as a hare does a greyhound.

It therefore ſeems to me, that ſwallows are by no means equal to long flights, from their practice during their ſummer reſidence with us.

I have long attended to the flight of birds; and it hath always appeared to me, that they are never on the wing for amuſement (as we walk or ride), but merely in ſearch of food.

The only bird which I have ever obſerved to fly without any particular point of direction, is the rook: theſe birds will, when the wind is high,

"Ride in the whirlwind, and enjoy the ſtorm."

They never fly, however, at this time, from point to point, but only tumble in the air, merely for their diverſion.

It ſeems, therefore, that birds are by no means calculated for flights acroſs oceans, for which they have no previous practice: and they are, in fact, always ſo fatigued, that, when they meet a ſhip at ſea, they forget all apprehenſions, and deliver themſelves up to the ſailors.

Let us now conſider another objection to the migration of the ſwallow, which Monſ. de Buffon ſuppoſes may croſs the Atlantic to the Line in eight days *; and this not only from the want of reſt, but of food, during the paſſage.

A ſwallow, indeed, feeds on the wing: but where is it to find any inſects, whilſt it is flying over a wide expanſe of ſea? This bird, therefore, if it ever attempted ſo adventurous a paſſage, would ſoon feel a want of food, and return again to land, where it had met with a conſtant ſupply from minute to minute.

I am aware it may be here objected, that the ſwallow leaves us on the approach of winter, when ſoon no flying inſects can be procured: but I ſhall hereafter endeavour to ſhew, that theſe birds are then torpid, and, conſequently, can want no ſuch food.

Another objection remains to the hypotheſis of migration, which is, that birds, when flying from

* Diſcours ſur la nature des oiſeaux, p. 32.

point

point to point, endeavour always to have the wind againſt them *, as is periodically experienced by the London bird-catchers, in March and October, when they lay their nets for ſinging birds †.

The reaſon, probably, for birds thus flying againſt the wind is, that their plumage may not be ruffled, which indeed I have before had occaſion to mention.

Let us ſuppoſe, then, a ſwallow to be equal to a paſſage acroſs the Atlantic in other reſpects; how is the bird to be inſured of the wind's continuing for days in the ſame quarter; or how is he to depend upon its continuing to blow againſt his flight with moderation? for who can ſuppoſe that a ſwallow can make his way to the point of direction, when buffeted by a ſtorm blowing in the teeth of his intended paſſage ‡?

Laſtly, can it be conceived that theſe, or any other birds, can be impelled by a providential inſtinct, regularly to attempt what ſeems to be attended with ſuch inſuperable difficulties, and what moſt frequently leads to certain deſtruction?

But it will ſtill be objected, that as ſwallows regularly appear and diſappear at certain ſeaſons, it is incumbent upon thoſe who deny their migration, to

* Kalm, in his voyage to America, makes the ſame obſervation, with regard to flying fiſh, and Valentine ſays, that if the wind does not continue to blow againſt the bird of paradiſe, it immediately drops to the ground.

† Theſe birds, as it ſhould ſeem, are then in motion; becauſe, at thoſe ſeaſons, the ground is plowed either for the winter or lent corn.

‡ I have myſelf attended to ſwallows during a high wind, and have obſerved that they fly only in ſheltered places, whilſt they almoſt touch the ſurface of the ground.

ſhew

ſhew what becomes of them in Europe during our winter.

Though it might be anſwered, that it is not neceſſary, thoſe who endeavour to ſhew the impoſſibility of another ſyſtem or hypotheſis, ſhould from thence be obliged to ſet up one of their own; yet I ſhall, without any difficulty, ſay, that I at leaſt am convinced ſwallows (and perhaps ſome other birds) are torpid during the winter.

I have not, I muſt own, myſelf ever ſeen them in this ſtate; but, having heard inſtances of their being thus found, from others of undoubted veracity, I have not ſcarcely the leaſt doubt with regard to this point.

It is, indeed, rather difficult to conceive why ſome ornithologiſts continue to withhold their aſſents to ſuch a cloud of witneſſes, except that it perhaps contradicts a favourite hypotheſis which they have already maintained.

Why is it more extraordinary that ſwallows ſhould be torpid during the winter, than that bats are found in this ſtate, and ſo many inſects, which are the food of ſwallows?

But it may be ſaid, that as the ſwallows have crowded the air during the ſummer, in every part of Europe ſince the creation, and as regularly diſappear in winter, why have not the inſtances of their being found in a torpid ſtate been more frequent?

To this it may be anſwered, that though our globe may have been formed ſo many centuries, yet the inhabitants of it have ſcarcely paid any attention to the ſtudy of natural hiſtory, but within theſe late years.

As for the ancient Greeks and Romans, their dreſs prevented their being ſo much in the fields as we are; or, if they heard of a rather extraordinary bird in their neighbourhood, they had not a gun to ſhoot it: the only method of attaining real knowledge in natural hiſtory, depends almoſt entirely upon the having frequent opportunities of thus killing animals, and examining them when dead.

If they did not ſtir much in their own country, much leſs did they think of travelling into diſtant regions; want of bills of exchange, and of that curioſity which ariſes from our being thoroughly acquainted with what is near us at home, probably occaſioned this; to which may alſo be added, the want of a variety of languages: ſcarcely any Greek ſeems to have known more than his own tongue, nor Roman more than two *.

Ariſtotle, indeed, began ſomething like a ſyſtem of natural hiſtory, and Pliny put down, in his common place-book, many an idle ſtory; but, before the invention of printing, copies of their works could not be ſo generally diſperſed, as to occaſion much attention to what might be intereſting facts for the natural hiſtorian.

In the ſixteenth century, Geſner, Belon, and Aldrovandus, publiſhed ſome materials, which might be of uſe to future naturaliſts; but, in the ſeventeenth, Ray and Willoughy firſt treated this extenſive branch of ſtudy, with that clearneſs of method,

* It need be ſcarcely here mentioned alſo, that their navigation was confined to the Mediteranean, from the compaſs not having been then diſcovered.

perſpicuity

perſpicuity of deſcription, and accuracy of obſervation, as hath not, perhaps, been ſince exceeded.

The works of theſe great naturaliſts were ſoon diſperſed over Europe, and the merit of them acknowledged; but it ſo happened, that Sir Iſaac Newton's amazing diſcoveries in natural philoſophy making their appearance about the ſame time, engaged entirely the attention of the learned.

In proceſs of time, all controverſy was ſilenced by the demonſtration of the Newtonian ſyſtem; and then the philoſophical part of Europe naturally turned their thoughts to other branches of ſcience.

Since this period, therefore, and not before, natural hiſtory hath been ſtudied in moſt countries of Europe; and conſequently, the finding ſwallows in a ſtate of torpidity, or on the coaſt of Senegal, during the winter, begins to be an intereſting fact, which is communicated to the world by the perſon who obſerves it.

To this I may add, that the common labourers, who have the beſt chance of finding torpid birds, have ſcarcely any of them a doubt with regard to this point; and conſequently, when they happen to ſee them in this ſtate, make no mention of it to others; becauſe they conſider the diſcovery as neither uncommon or intereſting to any one.

Molyneux, therefore, in the Philoſophical Tranſactions *, informs us, that this is the general belief of the common people of Ireland, with regard to land-rails; and I have myſelf received the ſame anſwer from a perſon who, in December, found ſwallows torpid in the ſtump of an old tree.

* Phil. Tranſ. abr. Vol. II. p. 853.

Another

Another reaſon why the inſtances of torpid ſwallows may not be expected ſo frequently, is, that the inſtinct of ſecreting themſelves at the proper ſeaſon of the year, likewiſe ſuggeſts to them, it's being neceſſary to hide themſelves in ſuch holes and caverns, as may not only elude the ſearch of man, but of every other animal which might prey upon them; it is not therefore by any common accident that they are ever diſcovered in a ſtate of torpidity.

Since the ſtudy of natural hiſtory, however, hath become more general, proofs of this fact are frequently communicated, as may appear in the Britiſh Zoology *.

That it may not be ſaid, however, I do not refer to any inſtance which deſerves credit, if properly ſifted, I beg leave to cite the letter from Mr. Achard to Mr. Collinſon, printed in the Philoſophical Tranſactions †, from whence it ſeems to be a moſt irrefragable fact, that ſwallows ‡ are annually diſcovered in a torpid ſtate on the banks of the Rhine. I ſhall alſo refer to Dr. Birch's Hiſtory of the Royal Society ||, where it is ſtated, that the celebrated Harvey diſſected

* See Vol. II. p. 250. Brit. Zool. ill. p. 13, 14. As alſo Mr. Pennant's Tour in Scotland, p. 199.

† 1763, p. 101.

‡ "Swallows or martins," are Mr. Achard's words, which I the rather mention, becauſe Mr. Collinſon complains that the ſpecies is not ſpecified.

Mr. Collinſon himſelf had endeavoured to prove, that ſand martins are not torpid, Phil. Tranſ. 1760, p. 109. and concludes his letter, by ſuppoſing that all the ſwallow tribe migrates, therefore the ſwift is the only ſpecies remaining; for his friend Mr. Achard ſhews to demonſtration, that ſwallows or martins are torpid; he does not, indeed, preciſely ſtate which of them.

|| Vol. IV. p. 537.

 ſome,

ſome, which were found in the winter, under water, and in which he could not obſerve any circulation of the blood *.

Aſſuming it, therefore, from theſe facts, that ſwallows have been found in ſuch a ſtate, I would aſk the partiſans of migration, whether any inſtance can be produced where the ſame animal is calculated for a ſtate of torpidity and, at the ſame time of the year, for a flight acroſs oceans?

But it may be urged, poſſibly, that if ſwallows are torpid when they diſappear, the ſame thing ſhould happen with regard to other birds, which are not ſeen in particular parts of the year.

To this I anſwer, that this is by no means a neceſſary inference: if, for example, it ſhould be inſiſted that other birds beſides the cuckow are equally careleſs with regard to their eggs, it would be immediately allowed that the argument ariſing from

* As the ſwallows were found in the winter, they muſt have been in a ſtate of torpidity, as otherwiſe the animals muſt have been putrid.

I ſhall likewiſe here refer to Phil. Tranſ. abr. Vol. V. p. 33. where Mr. Derham ſays, that he heard a ſwift ſqueak in an hole of his houſe on the 17th of April; but that, the weather being cold, it did not ſtir abroad for ſeveral days.

This ſeems to be a ſtrong inſtance of a bird's firſt waking from a ſtate of torpidity, but reſuming its ſleep on the weather being ſevere.

I ſhall cloſe the proofs on this head (which I could much enlarge) by the dignified teſtimony of Sigiſmond, King of Poland, who affirmed on his oath, to the cardinal Commendon, that he had frequently ſeen ſwallows, which were found at the bottom of lakes. See the life of cardinal Commendon, p. 211. Paris, 1671. 4to.

ſuch ſuppoſed analogy could by no means be relied upon *.

It is poſſible, however, that ſome other birds, which are conceived to migrate, may be really torpid as well as ſwallows; and if it be aſked why they are not ſometimes alſo ſeen in ſuch a ſtate during the winter, the anſwer ſeems to be, that perhaps there may be a thouſand ſwallows to any other ſort of bird, and that they commonly are found torpid in cluſters.

* I here ſuppoſe the common notion about the cuckow to be true; becauſe both learned and ignorant ſeem equally to agree in the fact.

During the preſent ſummer, however, a girl brought a full feathered young cuckow to a gentleman's houſe, where I happened to be, who ſaid, that it had been for ſeveral days before fed by another bird of equal ſize with itſelf; which therefore could not be a hedge-ſparrow, or other ſmall bird, but the parent cuckow.

I have alſo lately been favoured, by Mr. Pennant, with the following extract from a manuſcript of Derham's on inſtinct.

" The Rev. Mr. Stafford was walking in Gloſſop-dale in the " Peak of Derbyſhire, and ſaw a cuckow riſe from its neſt, " which was on the ſtump of a tree, that had been ſome time " felled, ſo as much to reſemble the colour of the bird. In " this neſt were two young cuckows, one of which he " faſtened to the ground, by means of a peg and line, and very " frequently, for many days, beheld the old cuckow feed theſe " her young ones."

It is not impoſſible, therefore, that this moſt general opinion will turn out like the ſuppoſed effects of the venom of the tarantula; and, indeed, it is difficult to conceive how ſo ſmall a bird as a hedge-ſparrow can feed a cuckow: it is alſo remarkable, that the witneſſes often vary about the ſpecies of ſmall bird thus employed.

It is poſſible, however, that the cuckow (though it may not hatch its young) may ſeed them, when grown too large for the foſter parent.

If a ſingle bird of any other kind happens to be ſeen in the winter, without motion or apparent warmth, it is immediately conceived that it died by ſome common accident.

I ſhall, however, without any reſerve, ſay, that I rather conceive the notion which prevails with regard to the migration of many birds, may moſt commonly ariſe from the want of obſervation, and ready knowledge of them, when they are ſeen on the wing, even by profeſſed ornithologiſts.

It is an old ſaying, that "a bird in the hand is "worth two in the buſh;" and this holds equally with regard to their being diſtinguiſhed, when thoſe even who ſtudy natural hiſtory, have but a tranſient ſight of the animal *.

If, therefore, a bird, which is ſuppoſed to migrate in the winter, paſſes almoſt under the noſe of a Linnæan, he pays but little attention to it, becauſe he cannot examine the beak, by which he is to claſs the bird. Thus I conceive, that the ſuppoſing a nightingale to be a bird of paſſage ariſes from not readily diſtinguiſhing it, when ſeen in a hedge, or on the wing †.

This bird is known to the ear of every one, by its moſt ſtriking and capital notes, but to the eye of very

* An ingenious friend of mine makes always a very proper diſtinction between what he calls in-door and out-door naturaliſts.

Thomas Williſel, who aſſiſted Ray and Willughby much with regard to the natural hiſtory of the animals of this iſland, never ſtirred any where without his gun and fiſhing-tackle.

† No two birds fly in the ſame manner, if their motions are accurately attended to.

few indeed; becauſe the plumage is dull, nor is there any thing peculiar in its make.

The nightingale ſings perhaps for two months *, and then is never heard again till the return of the ſpring, when it is ſuppoſed to migrate to us from the continent, with redſtarts, and ſeveral other birds.

That it cannot really do ſo, ſeems highly probable, from the following reaſons.

This bird is ſcarcely ever ſeen to fly above twenty yards, but creeps at the bottom of the hedges, in ſearch of maggots, and other inſects, which are found in the ground.

If the ſwallow is not ſupplied with any food during its paſſage acroſs oceans, much leſs can the nightingale be ſo accommodated; and I have great reaſon to believe, from the death of birds in a cage, which have had nothing to eat for twenty-four hours, that theſe delicate and tender animals cannot ſupport a longer faſt, though uſing no exerciſe at all.

To this I may alſo add, that thoſe birds which feed on inſects are vaſtly more feeble than thoſe whoſe bills can crack ſeed, and conſequently, leſs capable of bearing any extraordinary hardſhips or fatigue.

But other proofs are not wanting, that this bird cannot migrate from England.

* Whilſt it ſings even, the bird can ſeldom be diſtinguiſhed, becauſe it is then almoſt perpetually in hedges, when the foliage is thickeſt, upon the firſt burſt of the ſpring, and when no inſects can as yet have deſtroyed conſiderable parts of the leaves.

Nightin-

Nightingales are very common in Denmark, Sweden, and Ruſſia *, as alſo in every other part of Europe, as well as Aſia, if the Arabic name is properly tranſlated.

Now, if it is ſuppoſed that many of theſe birds which are obſerved in the ſouthern parts of England, croſs the German ſea, from the oppoſite coaſt of the continent; why does not the ſame inſtinct drive thoſe of Denmark to Scotland, where no ſuch bird was ever ſeen or heard †?

But theſe are not all the difficulties which attend the hypotheſis of migration; nightingales are agreed to be ſcarcely ever obſerved to the weſtward of Dorſetſhire, or in the principality of Wales ‡, much leſs in Ireland.

I have alſo been informed, that theſe birds are not uncommon in Worceſterſhire, whereas they are exceſſively rare (if found at all) in the neighbouring county of Hereford.

Whence, therefore, can it ariſe, that this bird ſhould at one time be equal to the croſſing of ſeas, and at other times not travel a mile or two into an adjacent county? Does it not afford, on the other hand, a ſtrong proof, that the bird really continues

* See Dr. Birch's Hiſtory of the Royal Society, Vol. III. p. 189. Linnæi Fauna Suecica. and Biographia Britannica, art. FLETCHER; where it is ſaid, that they have in Ruſſia a greater variety of notes than elſewhere.

† Sir Robert Sibbald, indeed, conceives the nightingale to be a bird of North Britain; but, if I can depend upon many concurrent teſtimonies, no ſuch bird is ever ſeen or heard ſo far northward at preſent, nor could I ever trace them in that direction further than Durham.

‡ I have, however, frequently ſeen the nightingale's congener (and ſuppoſed fellow-traveller) the redſtart in Wales.

on

on the ſame ſpot during the whole year, but happens not to be attended to, from the reaſons I have before ſuggeſted?

I am therefore convinced, that if I was ever to live in the country during the winter, I ſhould ſee nightingales, becauſe I ſhould be looking after them, and I am accordingly informed, by a perſon who is well acquainted with this bird, that he hath frequently obſerved them during this ſeaſon *.

If it be aſked, why the nightingales are all this time mute? the anſwer is, that the ſame ſilence is experienced in many other birds, and this very muteneſs is, in part the cauſe why the bird is not attended to in winter.

I muſt now aſk thoſe who contend for the migration of a nightingale, what is to be its inducement for croſſing from the continent to us? a ſwallow, indeed, may want flies in winter, if it ſtays in England; but a nightingale is juſt as well ſupplied with inſects on the continent, as it can be with us after its paſſage †. I muſt alſo aſk, in what other part of

* I find they have alſo been ſeen in France during the winter. See a treatiſe, intitled, Aëdologue, Paris 1751. p. 23.

† I have omitted the mention of a more minute proof, that this bird cannot migrate from the continent, from the having kept them for ſome years in a cage, and having been very attentive to their ſong.

Kircher (in his Muſurgia) hath given us the nightingale's notes in muſical characters, from which it appears that the ſong of a German nightingale differs very materially from that of an Engliſh one: now, if there was a communication by migration between the continent and England, the ſong of theſe birds would not ſo materially differ, as I may, perhaps, ſhew, by ſome experiments I have made, in relation to the notes of birds.

I have before mentioned, that Mr. Fletcher, who was embaſſador from England to Ruſſia in the time of Queen Elizabeth,

the

the world this bird is seen during the winter? must it migrate to Senegal with the swallow?

I am persuaded likewise, that the cuckow never migrates from this island any more than the nightingale: this bird is either probably torpid in the winter, or otherwise is mistaken for one of the smaller kind of hawks *; which it would be likewise in the spring, was it not for its very particular note at that time, and which only lasts during courtship, as it does with the quail.

If there is fine weather in February, this bird sometimes makes this sort of call to its mate, whilst it is supposed to continue still on the continent.

An instance is mentioned by Mr. Bradley †, of not only a single cuckow, but several, which were heard in Lincolnshire, during the month of February; and that able naturalist Mr. Pennant informs me, another was heard near Hatcham in Shropshire, on the 4th of February in the present year ‡.

observed that the song of the Russian nightingale differed from that of the English.

* Mr. Hunter, F. R. S. informs me, that he hath seen cuckows in the island of Belleisle during the winter, which is not situated so much to the southward, as to make it improbable that they may equally continue with us.

† Works of Nature, p. 77.

‡ Mr. Pennant received this account from Mr. Plimly, of Longnor in Shropshire.

Thus likewise Mr. Edwards informs us, that the sea fowls near the Needles, which are commonly supposed to migrate in winter, appear upon the weather's being very mild. Essays, p. 197.

It

It is amazing how much the being intereſted to diſcover particular objects contributes to our readily diſtinguiſhing them.

I remember the being much ſurprized that a grey-headed game-keeper always ſaw the partridge on the ground before they roſe, when I could not do the ſame. He told me, however, that the reaſon was, I lived in a time when the ſhooter had no occaſion to give himſelf that trouble.

He then further explained himſelf, by ſaying, that when he was young, no one ever thought of aiming at a bird when on the wing, and conſequently they were obliged to ſee the game before it was ſprung. He added, that from this neceſſity he could not only diſtinguiſh partridges, but ſnipes and woodcocks, on the ground.

Another inſtance of the ſame kind, is the great readineſs with which a perſon, who is fond of courſing, finds a hare ſitting in her form: thoſe, however, who are not intereſted about ſuch ſport, can ſcarcely ſee the hare, when it is under their noſe, and pointed out to them.

But more apparent objects eſcape our notice, when we are not intereſted about them.

Aſk any one, who hath not a botanical turn, what he hath ſeen in paſſing through a rich meadow, at the time it is moſt enamelled with plants in flower; and he will tell you, that he hath obſerved nothing but graſs and daiſies. If moſt gardeners even are in like manner aſked whether the flowers of a bean grow on every ſide of the ſtalk, they will ſuppoſe that they do,

whereas they, in reality, are only to be found on one side.

The mouths of flounders are often turned different ways, which one would think could not well escape the observation of the London fishmongers; yet, upon asking several of them whether they had attended to this particular, I found they had not, till I shewed them the proof in their own shops.

A fishmonger, however, knows immediately whether a fish is in good eating order or not, on the first inspection; because this is a circumstance which interests him.

I shall, however, by no means suppress two arguments in favour of migration, which seem to require the fullest answer that can be given to them.

The first is, that there are certain birds, which appear during the winter, but disappear during the summer; and it may be asked, where such birds can be supposed to breed, if they do not migrate from this island.

These birds are in number four, viz. the snipe, woodcock, redwing, and fieldfare.

As for the snipe, I have a very short answer to give to the objection, as far as it relates to this bird; because it constantly breeds in the fens of Lincolnshire, Wolmar forest, and Bodmyn downs; it is therefore highly probable, that it does the same in almost every county of England.

I must own, however, that, till within these few years, I conceived the nest of a snipe was as rarely seen in England, as that of a woodcock or fieldfare; and that able ornithologist Mr. Edwards supposes this to

be

be the fact, in the late publication of his ingenious Essays on Natural History *.

Woodcocks likewise are known to build in some parts of England every year; but, as the instances are commonly those of a single nest, I would by no means pretend to draw the same proof against the summer migration of this bird, as in the former case of the snipe.

I will most readily admit, that these accidental facts are rather to be accounted for, perhaps, from the whimsy or silliness of a few birds, which occasions their laying their eggs in a place where they are easily discovered, and contrary to what is usual with the bulk of the species.

I remember to have seen a duck's nest once on the top of a pollard willow, near the decoy in St. James's Park; it would not be, however, fair to infer from such an instance, that all ducks would pitch upon the same very improper situation for a nest, upon which it is difficult to conceive how a web-footed bird could settle.

Some silly birds likewise now and then choose a place for building, which cannot escape the observation of either man or beast, as he passes by.

I therefore suppose that the few proofs of woodcocks nests having been found in England, arise either from one or other of these two causes, and all which they seem to prove is, that our climate in summer is not absolutely improper for them.

It is to be observed, however, that Mr. Catesby considers such instances as of equal force against the

* P. 72.

migration of the woodcock, as of the fnipe *. Willughby alfo fays, that Mr. Jeffop faw young woodcocks fold at Sheffield (which rather implies a certain number being brought to market), and that others had obferved the fame elfewhere †.

We are, indeed, informed by Scopoli ‡, that they breed conftantly in Carniola, which is confiderably to the fouthward of any part of England: our country is therefore certainly not too hot for them.

Woodcocks appear and difappear almoft exactly about the fame time in every part of Europe, and perhaps Africa ‖: heat and cold, therefore, feem not to have any operation whatfoever with regard to the fuppofed migration of this bird.

But it may be faid, what fignifies proving the probability of woodcocks breeding in England, if it is not a known fact that they do fo?

To this it fhould feem there are feveral anfwers, as it is equally incumbent upon thofe who contend for migration, to fhew that thefe birds were ever feen on fuch paffage.

Another anfwer is, afk ninety-nine people out of a hundred, whether fnipes ever make a neft in England; and they will immediately fay, that they do not; fo little are facts or obfervations of this fort attended to.

But I fhall now endeavour to give fome other reafons why woodcocks may not only continue with us

* Phil. Tranf. abr. Vol. II. p. 889.
† B. iii. c. 1.
‡ Ornith. Leipfig, 1769.
‖ Shaw's Trav. Phyf. Obf. ch. ii.

during

during the ſummer, but alſo breed in large tracts of wood or bog, without being obſerved.

In the other parts of Europe, all birds almoſt are conſidered as game, or, at leaſt, are eaten as wholeſome food, Ray therefore mentions, that hawks and owls are ſold by the poulterers at Rome; every ſort of ſmall bird alſo is equally the foreign fowler's object *.

An Engliſhman does not conſider, on the other hand, perhaps twelve kinds of birds worthy his attention, or expence of powder, none of which are ever ſhot in our woods during the ſummer, nor are birds then diſturbed by felling either coppice or timber.

But it will be ſaid, why are not woodcocks ſometimes ſeen, however, as they may be ſuppoſed to leave their cover in ſearch of food?

To this I anſwer, that woodcocks ſleep always in the daytime, whilſt with us in the winter, and feed only during the night †. Whenever a woodcock, therefore, is fluſhed, he is rouſed from his ſleep by the ſpaniel or ſportſman, and then takes wing, becauſe there are no leaves on the trees to conceal the bird.

Whoever hath looked attentively at a woodcock's eye, muſt ſee that, from the appearance of it, the

* In one of Boccace's Novels, a lover, who lives at Florence, dreſſes a falcon for the dinner of his miſtreſs. Giorna a V. Novel. IX.

† Almoſt all the wild fowl of the duck kind alſo ſleep in the daytime, and feed at night.

ſight

fight muft be more calculated to diftinguifh objects by night than by day *.

The fact therefore is notorious to thofe who cut glades in their woods, and fix nets for catching thefe birds, that they never ftir but as it begins to be dark, after which they return again by day-break, when their fight even then is fo indifferent, that they ftrike againft the net, and thus become entangled.

No one with us ever thinks of fixing or attending fuch nets in fummer for woodcocks, becaufe it is not then fuppofed that there is any fuch bird in the ifland; if they tried this experiment, however, I muft own that I believe they would have fport †.

Mr. Reinhold Forfter, F. R. S. who is an able naturalift, informs me, that the fowlers in the neighbourhood of Dantzick kill many woodcocks about St. John's day (or Midfummer), in the following man-

* I conceive alfo, it is from the eyes looking fo dull, that this bird is generally confidered as being fo foolifh: hence the Africans call the woodcock *hummar el hadgel*, or the partridge's afs. Shaw's Phyf. Obf. ch. ii.

† I would afk thofe who will probably laugh at the very idea of fuch fport (which I do not, however, abfolutely infure), whether, if I was to fend them to any part of the Britifh coaft to catch the true anchovy, or tunny fifh, they would not fuppofe equally that it was a fool's errand.

Notwithftanding, however, this incredulity, I can produce the authority of both Ray (Syn. Pifc. p. 107.) and Mr. Pennant (Brit. Zool. III. p. 34. 36.), that the true anchovy is caught in the fea not far from Chefter, and the tunny fifh on the coaft of Argylefhire, together with the herrings, where they are called *mackrel fture*.

Is it not amazing, however, that a fifh of fuch a fize as the tunny fhould never have been heard of, even by the Scotch naturalift Sir Robert Sibbald?

ner,

ner, and that they continue to do ſo till the month of Auguſt.

They wait on the ſide of ſome of the extenſive woods in that neighbourhood, before day-break, for the return of the woodcock from his feeding in the night-time, and always depend upon having a very good chance of thus ſhooting many of them.

The Dantzickers, however, might be employed the whole ſummer near theſe woods in the day-time, without ever ſeeing ſuch a bird; and it ſeems therefore not improbable, that it ariſes from our not waiting for them at twilight or day-break, that they are never obſerved by Engliſhmen in the ſummer. If this bird ſhould, however, be ſeen in the night, it is immediately ſuppoſed to be an owl, which a woodcock does not differ much from in its flight.

To theſe reaſons for woodcocks not being obſerved, it may be added, that the bird is believed to be abſolutely mute, and conſequently, never diſcovers itſelf by its call.

If it be ſtill contended, that the neſt or young muſt ſometimes be ſtumbled upon, though in the centre of extenſive woods, or large bogs, the ſiſkin (or aberdavine *) is a much more extraordinary inſtance of concealing its neſt and young.

The plumage of this bird is rather bright than otherwiſe; and the ſong, though not very pleaſing, yet is very audible, both which circumſtances ſhould diſcover it at all times; yet Kramer † informs us, that, though immenſe numbers breed annually on

* Brit. Zool. p. 309.

† Elenchus Animalium per Auſtriam, p. 261. Viennæ, 1756.

 the

the banks of the [illegible], no one ever obſerved the neſt.

This bird is rather uncommon in England; ſo that if I aſk, whether the neſt was ever found within the [illegible] of the iſland, it may be conſidered as rather an unfair challenge.

There is another bird, however, called a red-poll *, which is taken in numbers during the Michaelmas and March flight, by the London bird-catchers, whoſe neſt, I believe, was never diſcovered in England, though I have ſeen them in pairs during the ſummer, both in the mountainous parts of Wales and highlands of Scotland †.

But I ſhall now mention another proof that woodcocks breed in England.

The Reverend Mr. White, of Selborn, who is not only a well-read naturaliſt, but an active ſportſman, informs me, that he hath frequently killed woodcocks in March, which, upon being opened, had the rudiments of eggs in them, and that it is uſual at that time to fluſh them in pairs. Willughby alſo obſerves the ſame ‡.

This bird, therefore, certainly pairs before its ſuppoſed migration; and can it be conceived that this ſtrict union (which birds in a wild ſtate ſo faithfully adhere to) ‖, ſhould take place before they

* Brit. Zool. p. 312.

† This elegant little bird is very common in Hudſon's Bay, where it feeds chiefly on the birch trees; which being more common in the northern than ſouthern parts of Great Britain, may account for the bird's being more often ſeen northward.

‡ B. III. c. i.

‖ It is believed that no mule-bird was ever ſeen in a wild ſtate, notwithſtanding M. de Buffon ſuſpects many an intrigue

traverſe

traverſe oceans, and when they cannot as yet have pitched upon a proper place for concealing their neſt and neſtlings?

Let us examine if this intercourſe before migration takes place in other birds, which are ſuppoſed to croſs wide extents of ſea: and a quail affords ſuch proof.

I have been preſent when theſe birds have been caught in the ſpring, which always turn out to be males, and are enticed to the nets by the call of the hen; quails therefore pair after they appear in England.

But I ſhall now conſider the other two inſtances of birds which are ſeen with us in the winter, and are not obſerved in the ſummer; I mean, the fieldfare and redwing.

And firſt, let us examine, where theſe birds are actually known to breed: the northern naturaliſts ſay, in Sweden; Klein, in the neighbourhood of Dantzick, which is only in lat. 54° 30′ *; and Willughby, in Bohemia.

in the receſſes of the woods (Hiſt. Nat. des Oiſeaux, tom. I.) ſuch irregular intercourſe is only obſerved in cages and aviaries, where birds are not only confined, but pampered with food.

* See Klein, de Avibus Erraticis, p. 178. Klein, however, cites Zornius, who lived in the ſame part of Germany, and who aſſerts that the *turdus Iliacus* (or redwing) leaves thoſe parts in the ſpring. The circumſtance therefore of the redwing's breeding in numbers *(per multitudines)* had eſcaped the notice of Zornius, though he hath written a diſſertation on this queſtion.

Is it at all ſurprizing, after this, that ſuch diſcoveries, if made at all, ſhould not be commonly heard of?

As they therefore build their neſts in more Southern parts of Europe, there is certainly no natural impoſſibility of their doing ſo with us, though, I muſt own, I never yet heard but of one inſtance, which was a fieldfare's neſt found near Paddington *.

I cannot, however, but think it is only from want of obſervation, that more of ſuch neſts have not been diſcovered, which are only looked after by very young children; and the chief object is the eggs, or neſtlings, not the bird which lays them †.

The plumage therefore and flight of the fieldfare or redwing being neither of them very remarkable, it is not at all improbable they may remain in ſummer, without being attended to; and particularly the redwing, which ſcarcely differs at all in appearance from other thruſhes. Thus the cough is by no means peculiar to Cornwall, as is commonly ſuppoſed, but is miſtaken for the jackdaw, or rook.

But it may be ſaid, that theſe birds fly in flocks during the winter, and if they remain here during the ſummer, we ſhould ſee them equally congregate.

I have not before referred to Klein, who hath written a very able treatiſe, in which he argues againſt the poſſibility of migration in birds; becauſe, though I ſhould be very happy to ſupport my poor opinion by his authority, yet I thought it right neither to repeat his facts, or arguments.

* See alſo Harl. Miſc. Vol. II. p. 561.

† Many birds alſo build in places of ſuch difficult acceſs, that boys cannot climb to; birds neſting is confined almoſt entirely to hedges, and low ſhrubs.

This

This circumſtance, however, is by no means peculiar to the fieldfare and redwing; moſt of the hard-billed ſinging birds do the ſame in winter, but ſeparate in ſummer, as it is indeed neceſſary all birds ſhould during the time of breeding.

I ſhall now conſider another argument in favour of migration, which I do not know hath been ever inſiſted upon by thoſe writers who have contended for it, and which at firſt appearance ſeems to carry great weight with it.

There are certain birds, which are ſuppoſed to viſit this iſland only at diſtant intervals of years; the Bohemian chatterer and croſs-bill * (for example) once perhaps in twenty.

The fact is not diſputed, that ſuch birds are not commonly obſerved in particular ſpots from year to year; but this may ariſe from two cauſes, either a partial migration within the verge of our iſland, or perhaps more frequently from want of a ready knowledge of birds on the wing, when they happen to be ſeen indeed, but cannot be examined.

I never have diſputed ſuch a partial migration; and indeed I have received a moſt irrefragable proof of ſuch a flitting, from the Rev. Mr. White of Selborn in Hampſhire, whoſe accurate obſervations I have before had occaſion to argue from.

* This bird changes the colour of its plumage at different ſeaſons of the year, which is ſometimes red.

The firſt account we have of their being ſeen, is in the Ph. Tr. abr. Vol. V. p. 33. where Mr. Edward Lhwyd ſuſpects them to be Virginia nightingales, from their feathers being red, and had no difficulty of at once ſuppoſing that they had croſſed the Atlantic.

The rock (or ring-ouzel) hath always hitherto been considered as frequenting only the more mountainous parts of this island: Mr. White, however, informs me that there is a regular migration of these birds, which flock in numbers, and regularly visit the neighbourhood of Selborn, in Hampshire *.

I therefore have little doubt but that they equally appear in others of our Southern counties; though it escapes common observation, as they bear a sort of general resemblance to the black-bird, at least to the hen of that species.

I own also, that I always conceived the Bohemian chatterer was not observed in Great Britain but at very distant intervals of years, and then perhaps only a single bird, whereas Dr. Ramsey (professor of natural history at Edinburgh) informs Mr. Pennant, that flocks of these birds appear constantly every year in the neighbourhood of that city †.

As for cross-bills, they are seen more and more in different parts of England, since there have been so many plantations of firs: this bird is remarkably fond of the seeds of these trees, and therefore changes its place to those parts where it can procure the greatest plenty of such food ‡.

* See also Br. Zool. III. p. 56.

† These birds are said to be particularly fond of the berries of the mountain-ash, which is an uncommon tree in the Southern parts of Great Britain, but by no means so in the North.

‡ This bird should also, for the same reason, be found from year to year in the cyder counties, if it was true (as is commonly supposed) that he is particularly fond of the kernels of

This

This flitting therefore by no means amounts to a total and periodical migration over ſeas, but is no more than what is experienced with regard to ſeveral birds.

For example, the Britiſh Zoology informs us *, that, at an average, 4000 dozen of larks are ſent up from the neighbourhood of Dunſtable, to ſupply the London markets; nor do I hear, upon inquiry, that there is any complaint of the numbers decreaſing from year to year, notwithſtanding this great conſumption.

I ſhould not ſuppoſe that 50 dozen of ſkylarks are caught in any other county of England; and it ſhould therefore ſeem that the larks from the more adjacent parts croud in to ſupply the vacuum occaſioned by the London Epicures, which may be the cauſe poſſibly of a partial migration throughout the whole iſland.

I begin now to approach to ſomething like a concluſion of this (I fear) tedious diſſertation: I think, however, that I ſhould not omit what appears to me at leaſt as a demonſtration, that one bird, which is commonly ſuppoſed to migrate acroſs ſeas, cannot poſſibly do ſo.

apples, which it is conceived he can inſtantly extract with his very ſingular bill.

Mr. Tunſtall, F. R. S. however, at my deſire, once placed an apple in the cage of a croſs-bill, which he had kept for ſome time in his very valuable and capital collection of live birds: upon examining the apple a fortnight afterwards, it remained untouched.

* P. 235.

A landrail

A landrail *, when put up by the ſhooter, never flies 100 yards; its motion is exceſſively ſlow, whilſt the legs hang down like thoſe of the water fowls which have not web feet, and which are known never to take longer flights.

This bird is not very common with us in England, but is exceſſively ſo in Ireland, where they are called corn-creaks.

Now thoſe who contend that the landrail, becauſe it happens to diſappear in winter, muſt migrate acroſs oceans, are reduced to the following dilemma.

They muſt firſt either ſuppoſe that it reaches Ireland periodically from America; which is impoſſible, not only becauſe the paſſage of the Atlantic includes ſo many degrees of longitude, but becauſe there is no ſuch bird in that part of the globe.

If the landrail therefore migrates from the continent of Europe to Ireland, which it muſt otherwiſe do, the neceſſary conſequence is, that many muſt paſs over England in their way Weſtward to Ireland; and why do not more of theſe birds continue with us, but, on the contrary, immediately proceed acroſs the St. George's channel?

Whence ſhould it ariſe alſo, if they paſs over this iſland periodically in the ſpring and autumn, that they are never obſerved in ſuch paſſage, as I have already ſtated their rate in flying to be exceſſively ſlow; to which I may add, that I never ſaw them riſe to the height of twenty yards from the ground, nor indeed exceed the pitch of a quail.

* Br. Zool. p. 387.

I have now submitted the beſt anſwers that have occurred, not only to the general arguments for the migration of birds acroſs oceans, but alſo to the particular facts, which are relied upon as actual proofs of ſuch a regular and periodical paſſage.

Though I may be poſſibly miſtaken in many of the conjectures I have made, yet I think I cannot be confuted but by new facts, and to ſuch freſh evidence, properly authenticated, I ſhall moſt readily give up every point, which I have from preſent conviction been contending for.

I may then perhaps alſo flatter myſelf, that the having expreſſed my doubts with regard to the proofs hitherto relied upon, in ſupport of migration, may have contributed to ſuch new, and more accurate obſervations.

It is to be wiſhed, however, that theſe more convincing and deciſive facts may be received from iſlanders (the more diſtant from any land the better*) and not from the inhabitants of a continent; as it does not ſeem to be a fair inference, becauſe certain birds leave certain ſpots at particular times, that they therefore migrate acroſs a wide extent of ſea.

For example, ſtorks diſappear in Holland during the winter, and they have not a very wide tract of ſea between them and England; yet this bird never frequents our coaſts.

* I would particularly propoſe the iſlands of Madera and St. Helena; to theſe, I would alſo add the iſland of Aſcenſion (had it any inhabitants), as likewiſe Juan Fernandez, for the Pacifick ocean.

The ſtork, however, may be truely conſidered as a bird of paſſage, by the inhabitants of thoſe parts of Europe (wherever ſituated) to which it may be ſuppoſed to reſort during the winter, and where it is not ſeen during the ſummer.

I am, dear Sir,

Your moſt faithful,

humble ſervant,

Daines Barrington.

P. S.

P. S.

SINCE I sent to you my very long letter on the migration of birds, I have had an opportunity of examining the "Planches Enluminées," which are said to be published under M. de Buffon's inspection, and which seem to afford a demonstration of M. Adanson's inaccuracy in supposing either the roller, or swallows, which he caught in his ship, near the coast of Senegal, to be the same with those of Europe.

In the 8th of these plates, there is a coloured figure of a bird, called le rollier d'Angola, which agrees exactly with M. Adanson's description *; but he trusted too much to his memory, when he pronounced it to be the same with the Garrulus Argentoratensis of Willughby, and therefore supposed it to be on its passage to Europe.

This bird hath, indeed, in many respects, a very strong resemblance to the common roller of Europe, which is represented also in the Planches Enluminées, plate 486; but it differs most materially in the length of the two exterior feathers of the tail, as well as in the colour of the neck, which in the African roller is of a most bright green, and in the European of rather a dull blue.

In the 310th plate, there is likewise a coloured representation of the "Hirondelle a ventre roux du "Senegal," which specimen was possibly furnished by Monf. Adanson himself.

* Voyage au Senegal, p. 15. There is also another African bird, represented in the "Planches Enluminées," which might very easily, on a hasty inspection, be mistaken for the Garrulus Argentoratensis, viz. the Guepier a longue queue du Senegal. Pl. Enl. p. 314.

The roller of Angola is also engraved by Brisson, T. ii. pl. 7.

It very much resembles the European swallow, but the tail differs, as the forks (in the Senegal specimen) taper from the top of the two exterior feathers to the bottom, at three regular divisions, whereas in the European they are nearly of the same width throughout.

The convincing proof, however, that the "Hi-"rondelle a ventre roux du Senegal" differs from our chimney swallow is, that the rump is entirely covered with a bright orange or chesnut, which in the European swallow "is of a very lovely but dark "purplish blue colour *."

Having lately looked into Aristotle's Natural History, with regard to the cuckow, I take this opportunity also of enlarging on the doubts I have thrown out, in relation to the prevailing notion of this bird's nestlings being hatched and fed by foster parents.

I find that this most general opinion takes its rise from what is said by this father of natural history, in his ninth book, and twenty-ninth chapter.

Aristotle there asserts, that the cuckow does not build a nest itself, but makes use most commonly of those of the wood-pigeon, hedge-sparrow, lark, (which he adds are on the ground) as well as that of the χλωρις †, which is in trees.

Now, if we take the whole of this account together, it is certainly not to be depended upon; for the wood-pigeon ‡ and hedge-sparrow do not build upon the ground, and no one ever pretended to have

* See Willughby, p. 312.

† The χλωρις is rendered *luteola*; but, as there is no description, it is difficult to say what bird Aristotle here alludes to; Zinanni supposes it to be the greenfinch.

‡ The wood-pigeon, from its size, seems to be the only bird which is capable of hatching, or feeding, the young cuc-

found

found a cuckow's egg in the nest of a lark, which, indeed, is so placed.

I have before observed, that the witnesses often vary with regard to the bird in which the cuckow's egg is deposited *; and Aristotle himself, in the seventh chapter of his sixth book, confines the foster-parents to the wood-pigeon and hedge-sparrow, but chiefly the former.

If the age † of Aristotle is considered, when he began to collect the materials for his Natural History, by the encouragement of Alexander after his conquests in India ‡, it is highly improbable he should have written from his own observations. He therefore seems to have hastily put down the accounts of the persons who brought him the different specimens from most parts of the then known world.

Inaccurate, however, and contradictory as these reports often turn out, it was the best compilation which the ancients could have recourse to; and Pliny

kow; yet, if it is recollected that this bird lives on seeds, it is probable that the cuckow, whose nourishment is insects, would either be soon starved, or incapable of digesting what was brought by the foster-parent. This objection is equally applicable to the χλωρις, if it is our greenfinch.

* Thus Linnæus supposes it (in the Fauna Suecica) to be the white wagtail, which bird builds in the banks of rivers, or roofs of houses, (See Zinanni, p. 51.) where it is believed no young cuckow was ever found.

† He did not leave the school of Plato till the age of thirty-eight (or, as some say, forty); after which, some years passed before he became Alexander's preceptor, who was then but fourteen: nor could he have written his Natural History, probably, till twelve years after this, as Pliny states that specimens were sent to him by Alexander, from his conquests in India. Aristotle therefore must have been nearly sixty, when he began this great work, and consequently must have described from the observations of others.

‡ Pliny, L. viii. c. 16.

therefore professes only to abridge him, in which he often does not do justice to the original.

Whatever was asserted by Aristotle, is well known to have been most implicitly believed, till the last century; and I am convinced that many of the learned in Europe would, before that time, not have credited their own eyesight against what he had delivered.

There cannot be a stronger proof that the general notion about the cuckow arises from what is laid down by Aristotle, than the chapter which immediately follows, as it relates to the goatsucker, and states that this bird sucks the teats of that animal.

From this circumstance, the goatsucker hath obtained a similar name in most languages, though it is believed no one (who thinks at all about matters of this sort) continues to believe that this bird sucks the goat *, any more than the hedgehog does the cow.

I beg leave, however, to explain myself, that I give these additional reasons only for my doubting with regard to this most prevailing opinion; because I am truly sensible that many things happen in nature, which contradict all arguments from analogy, and I am persuaded, therefore, that the first person who gave an account of the flying fish, was not credited by any one, though the existence of this animal is not now to be disputed.

All that I mean to contend for is, that the instances of such extraordinary peculiarities in animals, should be proportionably well attested, in all the necessary circumstances.

I must own, for example, that nothing short of the following particulars will thoroughly satisfy me on this head.

* See Zinanni p. 95. who took great pains to detect this vulgar error.

The

The hedge-ſparrow's neſt muſt be found with the proper eggs in it, which ſhould be deſtroyed by the cuckow, at the time ſhe introduces her ſingle egg *.

The neſt ſhould then be examined at a proper diſtance from day to day, during the hedge-ſparrow's incubation, as alſo the motions of the foſter parent attended to, particularly in feeding the young cuckow, till it is able to ſhift for itſelf.

As I have little doubt that the laſt mentioned circumſtance will appear deciſive to many, without the others which I have required, it may be proper to give my reaſons, why I cannot conſider it alone, as ſufficient.

There is ſomething in the cry of a neſtling for food, which affects all kinds of birds, almoſt as much as that of an infant, for the ſame purpoſe, excites the compaſſion of every human hearer †.

I have taken four young ones from a hen ſkylark, and placed in their room five neſtling nightingales, as well as five wrens; the greater part of which were reared by the foſter parent.

It can hardly in this experiment be contended, that the ſkylark miſtook them for her own neſtlings, be-

* I could alſo wiſh that the following experiment was tried. When a hedge-ſparrow hath laid all her eggs, a ſingle one of any other bird, as large as a cuckow, might be introduced, after which if either the neſt was deſerted, or the egg too large to be hatched, it would afford a ſtrong preſumption againſt this prevailing opinion. I muſt here alſo take notice, that Mr. Hunter, F. R. S., who hath diſſected hen cuckows, informs me that they are not incapacitated from hatching their eggs, as hath been ſuppoſed by ſome ornithologiſts.

† I am perſuaded that a cuckow is oftener an orphan, than any other neſtling, becauſe, from the curioſity which prevails with regard to this bird, the parents are eternally ſhot.

cauſe

caufe they differed greatly, not only in number and fize, but in their habits, for nightingales and wrens perch, which a fkylark is almoft incapable of, though, by great affiduity, fhe at laft taught herfelf the proper equilibre of the body.

I have likewife been witnefs of the following experiment: two robins hatched five young ones in a breeding cage, to which five others were added, and the old birds brought up the whole number, making no diftinction between them.

The Aëdologie alfo mentions (which is a very fenfible treatife on the nightingale *) that neftlings of all forts may be reared in the fame manner, by introducing them to a caged bird, which is fupplied with the proper food.

Not only old birds, however, attend to this cry of diftrefs from neftlings, but young ones alfo which are able to fhift for themfelves.

I have feen a chicken, not above two months old, take as much care of younger chickens, as the parent would have fhewn to them which they had loft, not only by fcratching to procure them food, but by covering them with her wings; and I have little doubt but that fhe would have done the fame by young ducks.

I have likewife been witnefs of neftling thrufhes of a later brood, being fed by a young bird which was hatched earlier, and which indeed rather overcrammed the orphans intrufted to her care; if the bird however erred in judgement, fhe was certainly not deficient in tendernefs, which I am perfuaded fhe would have equally extended to a neftling cuckow.

* Paris, 1751, or 1771.

XXII. KON-

Received February 13, 1772.

XXII. ΚΟΣΚΙΝΟΝ ΕΡΑΤΟΣΘΕΝΟΥΣ.

O R,

The Sieve of Eratosthenes.

Being an account of his method of finding all the Prime Numbers, by the Rev. Samuel Horsley, *F. R. S.*

Read May 7, 1772.

A Prime number is such a one, as hath no intregral divisor but unity.

A number, which hath any other integral divisor, is Composite.

Two or more numbers, which have no common integral divisor, besides unity, are said to be Prime with respect to one another.

Two or more numbers, which have any common integral divisor besides unity, are said to be Composite with respect to one another.

The distinction of numbers into Prime and Composite, is so generally understood, that I suppose it is needless to enlarge upon it.

To determine, whether several numbers proposed be Prime or Composite *with respect to one another*, is an easy Problem. The solution of it is given by Euclid, in the three first propositions of the 7th book

book of the Elements, and is to be found in many common treatises of Arithmetic and Algebra. But to determine, concerning any number proposed, whether it be *absolutely* Prime or Composite, is a Problem of much greater difficulty. It seems indeed incapable of a direct solution, by any general method; because the successive formation of the prime numbers doth not seem reducible to any general law. And for the same reason, no direct method hath hitherto been hit upon, for constructing a Table of all the prime numbers to any given limit. Eratosthenes, whose skill in every branch of the philosophy and literature of his times, rendered his name so famous among the Sages of the Alexandrian School, was the inventor of an indirect method, by which such a table might be constructed, and carried to a great length, in a short time, and with little labour. This extraordinary and useful invention is at present, I believe, little, if at all, known; being described only by two writers, who are seldom read, and by them but obscurely; by Nicomachus Gerasinus, a shallow writer of the 3d or 4th century, who seems to have been led into mathematical speculations, not so much by any genius for them, as by a fondness for the mysteries of the Pythagorean and Platonic philosophy; and by Boethius, whose treatise upon numbers is but an abridgment of the wretched performance of Nichomachus*. I flatter myself therefore, that a succinct account of it will not be unacceptable to this learned Society.

* There are more pieces than one of this Nichomachus extant. That which I refer to is intitled [illegible].

But

But before I enter expreſsly upon the ſubject, I muſt take the liberty to animadvert upon a certain Table, which, among other pieces aſcribed to Eratoſthenes, is printed at the end of the beautiful edition of Aratus publiſhed at Oxford in the year 1672, and is adorned with the title of Κοσκινον Ερατοσθενυς. It contains all the odd numbers from 3 to 113 incluſive, diſtributed in little cells, all the diviſors of every Compoſite number being placed over it, in its proper cell, and the Prime numbers are diſtinguiſhed, ſo far as the table goes, by having no diviſors placed over them. It hath probably been copied either from a Greek comment upon the Arithmetic of Nicomachus, preſerved among the manuſcripts of Mr. Selden in the Bodleian Library, in which, though the manuſcript is now ſo much decayed as to be in moſt places illegible, I find plain veſtiges of ſuch a table *, which might be more perfect 100 years ago, when the Oxford Aratus was publiſhed; or elſe, from another comment, tranſlated from a Greek manuſcript into Latin, and publiſhed in that language, by Camerarius, in which a table of the very ſame form occurs, extending from the number 3 to 109 incluſive. It may ſufficiently ſkreen the editor of Aratus from cenſure, that he had theſe authorities to publiſh this table as the Sieve of Eratoſthenes; eſpecially as they are in ſome meaſure ſupported by paſſages of Nicomachus himſelf. But the Sieve of Eratoſthenes was quite another thing.

* This manuſcript ſeems to have contained the text of Nicomachus with Scholia in the margin. But the table evidently belongs to the Scholia, not to the text.

 The

The Oxford editor hath annexed to his table, to explain the uſe of it, ſome detached paſſages, which he hath ſelected from the text of Nicomachus, and from a comment upon Nicomachus aſcribed to Joannes Grammaticus. In theſe paſſages the difference between Prime and Compoſite numbers is explained, in many words indeed, but not with the greateſt accuracy; and it is propoſed to frame a kind of Table of all the odd numbers, from 3 to any given limit, in which the Compoſite numbers ſhould be diſtinguiſhed by certain marks *. The Primes would conſequently be characteriſed, as far as the table ſhould be carried, by being unmarked. But, upon what principles, or by what rule, ſuch a table is to be conſtructed, is not at all explained. It is obvious that, in order to *mark* the Compoſite numbers, it is neceſſary to know which are ſuch. And, without ſome rule to diſtinguiſh which numbers are Prime, and which are Compoſite, independent of any table in which they ſhall be diſtinguiſhed by marks, it is impoſſible to judge, whether the table be true, as far as it goes, or to extend it, if requiſite, to a further limit. Now it was the Rule by which the Prime numbers and the Compoſite might be diſtinguiſhed, not a Table conſtructed we know not how, that was the invention of Eratoſthenes, to which from its uſe, as well as from the nature of the operation, which

* Nicomachus and Joannes Grammaticus propoſe that theſe marks ſhould be ſuch, as ſhould not only diſtinguiſh the compoſite numbers, but likewiſe ſerve to expreſs all the diviſors of every ſuch number. It will be ſhewn, in a proper place, that this was no part of the original contrivance of the Sieve.

5 proceeds

proceeds (as will be ſhewn) by a gradual extermination of the compoſite numbers from the arithmetical ſeries 3. 5. 7. 9. 11. &c. infinitely continued, its author gave the name of the Sieve. I have thought it neceſſary to premiſe theſe remarks, to remove a prejudice, which I apprehend many may have conceived, as this beautiful and valuable edition of Aratus is in every ones hands, that this ill-contrived table, the uſeleſs work of ſome monk in a barbarous age, was the whole of the invention of the great Eratoſthenes, and in juſtice to myſelf, that I might not be ſuſpected of attempting to reap another's harveſt.

I now proceed, to give a true account of this excellent invention; which, for its uſefulneſs, as well as for its ſimplicity, I cannot but conſider as one of the moſt precious remnants of Ancient Arithmetic. I ſhall venture to repreſent it according to my own ideas, not obliging myſelf to conform, in every particular, to the account of Nicomachus, which I am perſuaded is in many circumſtances erroneous. In ſtating the principles upon which the Operation of the Sieve was founded, he hath added obſervations upon certain relations of the odd numbers to one another, which are certainly his own, becauſe they are of no importance in themſelves, and are quite foreign to the purpoſe. Every thing of this kind I omit: and having ſtated what I take to have been the genuine Theory of Eratoſthenes's method, cleared from the adulterations of Nicomachus, I deduce from it an operation of great ſimplicity, which ſolves the Problem in queſtion with wonderful eaſe, and which,

becaufe it is the moft fimple that the theory feems to afford, I fcruple not to adopt as the original Operation of the Sieve, though nothing like it is to be found in Nicomachus; though, on the contrary, Nichomachus, and all his Commentators, would fuggeft an operation very different from it, and far more laborious. For the fatisfaction of the curious and the learned, I have annexed a copy of fo much of Nicomachus's treatife, as relates to this fubject, with fuch corrections of the text, as it ftands in the edition of Wichelius, printed at Paris ann. 1538, as the fenfe hath fuggefted to me, or I have thought proper to adopt, upon the authority of a manufcript preferved among thofe of Archbifhop Laud, in the Bodleian Library; which, in this part, I have carefully collated. By comparing this with the account which I fubjoin, every one will be able to judge how far I have done juftice to the invention I have undertaken to explain.

PROBLEM.

To find all the Prime Numbers.

The number 2 is a Prime number; but, except 2, no even number is Prime, becaufe every even number, except 2, is divifible by 2, and is therefore Compofite. Hence it follows, that all the Prime numbers, except the number 2, are included in the feries of the odd numbers, in their natural order, infinitely extended; that is, in the feries

3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29. 31. 33. 35. 37. 39. 41. 43. 45. 47. 49. 51. &c.

Every

Every number which is not Prime, is a multiple of ſome Prime number, as Euclid hath demonſtrated (Element. 7. prop. 33.) Therefore the foregoing ſeries conſiſts of the Prime numbers, and of multiples of the Primes. And the multiples, of every number in the ſeries, follow at regular diſtances; by attending to which circumſtance, all the multiples, that is, all the Compoſite numbers, may be eaſily diſtinguiſhed and exterminated.

I ſay, the multiples of all numbers, in the foregoing ſeries, follow at regular diſtances.

For between 3 and its firſt multiple in the ſeries (9) two numbers intervene, which are not multiples of 3. Between 9 and the next multiple of 3 (15) two numbers likewiſe intervene, which are not multiples of 3. Again between 15 and the next multiple of 3 (21) two numbers intervene, which are not multiples of 3; and ſo on. Again, between 5 and its firſt multiple (15) four numbers intervene, which are not multiples of 5. And between 15 and the next multiple of 5 (25) four numbers intervene which are not multiples of 5; and ſo on. In like manner, between every pair of the multiples of 7, as they ſtand in their natural order in the ſeries, 6 numbers intervene which, are not multiples of 7. Univerſally, between every two multiples of any number n, as they ſtand in their natural order in the ſeries, $\overline{n-1}$ numbers intervene, which are not multiples of n.

Hence may be derived an Operation for exterminating the Compoſite numbers, which I take to have been the Operation of the Sieve, and is as follows.

The Operation of the Sieve.

Count all the terms of the ſeries following the number 3, by threes, and expunge every third number. Thus all the multiples of 3 are expunged. The firſt uncancelled number that appears in the ſeries, after 3, is 5. Expunge the ſquare of 5. Count all the terms of the ſeries, which follow the ſquare of 5, by fives, and expunge every fifth number, if not expunged before. Thus all the multiples of five are expunged, which were not at firſt expunged, among the multiples of 3. The next uncancelled number to 5 is 7. Expunge the ſquare of 7. Count all the terms of the ſeries following the ſquare of 7, by ſevens, and expunge every ſeventh number, if not expunged before. Thus all the multiples of 7 are expunged, which were not before expunged among the multiples of 3 or 5. The next uncancelled number which is now to be found in the ſeries, after 7, is 11. Expunge the ſquare of 11. Count all the terms of the ſeries, which follow the ſquare

---

3. 5. 7. ~~9~~. 11. 13. ~~15~~. 17. 19. ~~21~~. 23. ~~25~~. ~~27~~. 29. 31.
~~33~~. ~~35~~. 37. ~~39~~. 41. 43. ~~45~~. 47. ~~49~~. ~~51~~. 53. ~~55~~. ~~57~~. 59.
61. ~~63~~ ~~65~~. 67. ~~69~~. 71. 73. ~~75~~. ~~77~~. 79. ~~81~~. 83. ~~85~~. ~~87~~.
89. ~~91~~. ~~93~~. ~~95~~. 97. ~~99~~. 101. 103. ~~105~~. 107. 109. ~~111~~. 113.
~~115~~. ~~117~~. ~~119~~. ~~121~~. ~~123~~. ~~125~~. 127. ~~129~~. 131. ~~133~~. ~~135~~.
137. 139. ~~141~~. ~~143~~. ~~145~~. ~~147~~. 149. 151. ~~153~~. ~~155~~. 157.

of

of 11, by elevens, and expunge every eleventh number, if not expunged before. Thus all the multiples of 11 are expunged, which were not before expunged among the multiples of 3, 5, and 7. Continue these expunctions, till the first uncancelled number that appears, next to that whose multiples have been last expunged, is such, that its square is greater than the last and greatest number to which the series is extended. The numbers which then remain uncancelled are all the Prime numbers, except the number 2, which occur in the natural progression of number from 1 to the limit of the series. By the limit of the series I mean the last and greatest number to which it is thought proper to extend it.

Thus the prime numbers are found to any given limit.

Nicomachus proposes to make such marks over the Composite numbers, as should shew all the divisors of each. From this circumstance, and from the repeated intimations both of Nicomachus, and his commentator Joannes Grammaticus *, one would be led to imagine, that the Sieve of Eratosthenes was something more than its name imports, a method of sifting out the Prime numbers from the indiscriminate mass of all numbers Prime and Composite, and that, in some way or other, it exhibited all the divisors of every Composite number, and likewise shewed whether two or

* The Comment of Joannes Grammaticus is extant in manuscript in the Savilian Library at Oxford, to which I have frequent access, by the favour of the Reverend and Learned Mr. Hornsby, the Savilian Pofessor of Astronomy.

more

more Composite numbers were Prime or Composite with respect to each other. I have many reasons to think, that this was not the case. I shall as briefly as possible point out some of the chief, for the matter is not so important, as to justify my troubling the Society with a minute detail of them. First then, in the natural series of odd numbers, 3. 5. 7. &c. every number is a divisor of some succeeding number. Therefore if we are to have marks for all the different divisors of every Composite number, we must have a different mark for every odd number. Therefore we must have as many marks, or systems of marks, as numbers; and I do not see, that it would be possible, to find any more compendious marks, than the common numeral characters. This being the case, it would be impracticable to carry such a table as Nicomachus proposes, and his commentators have sketched, to a sufficient length to be of use, on account of the multiplicity of the divisors of many numbers, and the confusion which this circumstance would create *. It is hardly to be supposed, that Eratosthenes could overlook this obvious difficulty, though Nicomachus hath not attended to it. Eratosthenes therefore could not intend the construction of such a table.

In the next place, such a table not being had, Eratosthenes could not but perceive, that, the determining whether two or more numbers be Prime or Composite with respect to one another, is in all cases to be done more easily, by the direct method given by Euclid, than by

* The number 3465 hath no less than 22 different divisors.

the

the method of the Sieve. And he could not mean, to apply this method to a problem, to which another was better adapted.

Laftly, Eratofthenes could not mean, that the method of the Sieve fhould be applied to the finding of all the poffible divifors of any Compofite number propofed, becaufe he could not be unacquainted with a more ready way of doing this, founded upon two obvious Theorems, which could not be unknown to him.

The Theorems I mean are thefe.

1ft. If two Prime numbers multiply each other, the number produced hath no divifors but the two prime factors.

2d. If a Prime number multiply a Compofite number, and likewife multiply all the divifors of that compofite feverally, the numbers produced by the multiplications of thefe divifors will be divifors of the number produced by the firft multiplication: And the number produced by the firft multiplication will have no divifors, but the two factors, the divifors of the Compofite factor, and the numbers made by the multiplication of thefe divifors by the Prime factor feverally.

The method of finding all the divifors of any Compofite number, delivered by Sir Ifaac Newton in the Arithmetica Univerfalis, and by Mr. Maclaurin in his Treatife of Algebra, may be deduced from thefe propofitions, as every mathematician will eafily perceive. This method requires indeed that the leaft prime divifor fhould be previoufly found; and, if the leaft prime divifor fhould happen to be a large number, as it is not affignable by any general method, the

investigation of it by repeated tentations may be very tedious. A table therefore of the odd numbers *, in which the Composite numbers should each have its least Prime divisor written over it, would be very useful. But Nichomachus's project of framing a table in which each Composite number should have *all* its divisors written over it, is ridiculous and absurd, on account of the insuperable difficulties which would attend the execution of it.

Feb. 7, 1772.

S. Horsley.

* A table of the odd numbers would be sufficient: for the number 2 is the least prime divisor of every even number; and it is easy, even in the largest numbers, to try whether they are divisible by 2. In our method of notation, this may always be known, by observing the last figure in the expression of the number proposed.

EXCERPTA QUÆDAM

EX

Arithmeticâ Nicomachi

Ad Cribrum Eratosthenis pertinentia.

Ἡ δὲ τούτων φύσις (a), ὑπὸ Ἐρατοσθένους, καλεῖται Κόσκινον· ἐπειδὴ ἀναπεφυρμένους τοὺς περισσοὺς λαβόντες καὶ ἀδιακρίτους, ἐξ αὐτῶν [τὰ διαφέροντα ἀλλήλων εἴδη] (b) ταύτῃ τῇ τῆς φύσεως (c) μεθόδῳ διαχωρίζομεν, ὡς δι᾽ ὀργάνου ἢ κοσκίνου τινός· καὶ ἰδίᾳ μὲν τοὺς πρώτους καὶ ἀσυνθέτους, χωρὶς δὲ τοὺς μίκτους εὑρίσκομεν. Ἔστι δὲ ὁ τρόπος τοῦ Κοσκίνου τοιοῦτος. Ἐκθέμενος τοὺς ἀπὸ τριάδος πάντας ἐφεξῆς περισσούς, ὡς δυνατὸν μάλιστα ἐπὶ μήκιστον στίχον, ἀρξάμενος ἀπὸ τοῦ πρώτου, ἐπισκοπῶ τίνας οἷός τε ἔσται μετρεῖν ἕκαστος· καὶ εὑρίσκω δυνατὸν ὄντα τὸν πρῶτον, ἤτοι τὸν γ̄, τοὺς δύο μέσους διαλείποντας (d) μετρεῖν, μέχρις οὗ προχωρεῖν ἐθέλωμεν (e). οὐχ᾽ ὡς ἔτυχε δέ, καὶ εἰκῇ, μετροῦντα, ἀλλὰ τὸν μὲν πρῶτως αὐτῶν κείμενον, τοῦτ᾽ ἔστι τὸν ἀφ᾽ ἑαυτοῦ τοὺς δύο μέσους διαλεί-

(a) Mallem εὕρεσις, etsi, ne quid dissimulem, lectioni receptæ adstipulatur Boethii interpretatio.

(b) Voces uncis inclusas conjecturâ supplevi; quin et sequentium ordinem paululum immutavi, pro τῇ φύσεως μεθόδῳ ταύτῃ, scribendo ταύτῃ τῇ κ. τ. λ.

(c) Vocem φύσεως hic loci retinendam censeo. Locum integrum sic interpretor. "Suam horum indaginem Eratosthenes, Cribrum vocavit. Propterea quod imparibus universis, nullo generum discrimine, in medio collocatis, ipsam procreationem continuam, quo tradidit ille modo, insequendo [id est, procreationis continuæ, Eratosthenis modo, explorata lege] species diversas seorsim sistimus, cribro tanquam separatas."

(d) Cod. MS. habet διαλείποντα. Wechelius παραλείποντα.

(e) Ex Cod. MS. pro ἐθέλομεν.

πουντα (f), κατὰ τὴν τοῦ πρώτου ἐν τῷ στίχῳ κειμένου ποσότητα μετρήσει· τοῦτ᾽ ἔστι κατὰ τὴν ἑαυτοῦ, τρὶς γάρ· τὸν δ᾽ ἀπ᾽ ἐκείνου δύο διαλείποντα, κατὰ τὴν τοῦ δευτέρου τεταγμένου, πεντάκις γάρ· τὸν δὲ περαιτέρω πάλιν δύο διαλείποντα, κατὰ τὴν τοῦ τρίτου τεταγμένου, ἑπτάκις γάρ· τὸν δὲ ἔτι περαιτέρω ὑπὲρ δύο κείμενον, κατὰ τὴν τοῦ τετάρτου τεταγμένου, ἐννεάκις γάρ· καὶ ἐπ᾽ ἄπειρον τῷ αὐτῷ τρόπῳ. Εἶτα μετὰ τοῦτον, ἀπ᾽ ἄλλης ἀρχῆς, ἐπὶ τὸν δεύτερον ἐλθὼν, σκοπῶ τίνας οἷός τε ἔστι μετρεῖν· καὶ εὑρίσκω πάντας τοὺς τέσσαρας (g) διαλείποντας· ἀλλὰ τὸν μὲν πρῶτον, κατὰ τὴν ἐν τῷ στίχῳ πρώτου τεταγμένου ποσότητα· τρὶς γάρ. τὸν δὲ δεύτερον, κατὰ τὴν τοῦ δευτέρου· πεντάκις γάρ· τὸν δὲ τρίτον, κατὰ τὴν τοῦ τρίτου· ἑπτάκις γάρ· καὶ τοῦτο ἐφεξῆς ἀεί. Πάλιν δὲ ἄνωθεν, ὁ τρίτος, ὁ ζ, τὸ μετρεῖν* παραλαβὼν, μετρήσει τοὺς ἐξ διαλείποντας· ἀλλὰ τὸν μὲν πρώτιστον, κατὰ τὴν τοῦ γ (h) ποσότητα, πρώτου κειμένου· τὸν δὲ δεύτερον κατὰ τὴν τοῦ ε· δευτερόταγὴς γὰρ οὗτος (i). τὸν δὲ τρίτον, κατὰ τὴν τοῦ ζ, τρίτην γὰρ ἔχει (k) οὗτος τάξιν ἐν τῷ στίχῳ. καὶ, κατὰ τὴν αὐτὴν ἀναλογίαν, δι᾽ ὅλου (l) ἀπαραποδίστως (m) προχωρήσει σοι τοῦτο, ὥστε τὸ μὲν μετρεῖν διαδέξον"), κατὰ τὴν ἐν τῷ στίχῳ αὐτῶν ἐγκειμένην τάξιν· τὸ δὲ πόσους διαλείποντας,

(f) Locum in Editione Wechelii corruptum, in Cod. MS. mutilum & turbatum, conjecturâ, prout potui, sanatum dedi. Editio Wechelii habet τὸν τοὺς δύο μέσους ὑπερβαίνοντα. Codex MS. τὸν δύο. τελέσει τὸν τρία.

(g) Conjecturâ, pro τετράδι.

(h) Litera numeralem γ, conjecturâ posui pro voce τρια.

(i) Restitui ex Cod. MS pro ὄντος, quæ est Wechelii lectio.

(k) Particulam καὶ omisi.

(l) Wechelium sequor. Cod. MS. habet λογου, sensu, ut videtur, nullo.

(m) Ex Cod. MS. pro ἀπαριμπόδιστον.

* Conjecturâ pro μέτρον.

καταf

κατὰ τὴν ἀπὸ δυάδος ἐπ' ἄπειρον εὔτακτον τῶν (n) ἀρτίων προκοπὴν, ἢ κατὰ τὴν τ χώρας διπλασίασιν καθ' ἣν ὁ μέτρων τέτακται· τὸ δὲ ποσάκις, κατὰ τὴν τῶν ἀπὸ τριάδος περισσῶν εὔτακτον ἐπ' ἄπειρον (o) προχώρησιν (p). Ἐὰν οὖν σημείοις τισὶν ἐπισήξῃς τοὺς ἀριθμοὺς, εὑρήσεις τοὺς μὲν καταλαμβάνοντας τὸ μετρεῖν, οὔτε ἅμα πάντας τὸν αὐτόν ποτε μετροῦντας, ἔστι δὲ ὅτε οὐδὲ δύο τὸν αὐτόν· οὔτε πάντας ἁπλῶς τοὺς ἐκκειμένους ὑποπίπτοντας μέτρῳ τινὶ αὐτῶν. ἀλλὰ τινὰς μὲν παντελῶς διαφεύγοντας τὸ μετρηθῆναι ὑφ' οὑτινοσοῦν· τινὰς δὲ ὑφ' ἑνὸς μόνου μετρουμένους· τινὰς δὲ ὑπὸ δυὸ, ἢ καὶ πλειόνων. Οἱ μὲν οὖν μηδαμῶς (q) μετρηθέντες, ἀλλὰ διαφυγόντες τοῦτο, πρῶτοι εἰσὶ καὶ ἀσύνθετοι, ὡς ὑπὸ κοσκίνου διακριθέντες. οἱ δὲ ὑφ' ἑνὸς μόνου μετρηθέντες, κατὰ τὴν ἑαυτοῦ (r) ποσότητα, ἓν μόνον μόριον ἑτερώνυμον ἕξουσι πρὸς τῷ παρωνύμῳ. οἱ δὲ ὑφ' ἑνὸς μὲν (s), ἑτέρου δὲ ποσότητι, καὶ μὴ τῇ ἑαυτοῦ, ἢ ὑπὸ δύο ὁμοῦ μετρηθέντες, πλείονα ἕξουσι τὰ ἑτερώνυμα μέρη πρὸς τῷ παρωνύμῳ. οὗτοι οὖν ἔσονται

(n) Conjectura pro τὴν.

(o) Voces ἐπ' ἄπειρον ex Cod. MS. restitui.

(p) Nempe series numerorum imparium 3, 5, 7, 9, &c. infinite protensa, cum numeros impares universos contineat, imparis cujusvis multiplices omnes impares necessario complectitur. Est igitur n numerus quilibet impar. In serie 3, 5, 7, &c. infinite protensa, habes numeros omnes $n \times 3$, $n \times 5$, $n \times 7$, $n \times 9$, &c. Et cum seriei ea Lex sit & Conditio, ut naturali ordine numeri impares sequantur, & minor omnis numerus majorem praecedat, fieri nequit, quin multiplices numeri n cum inter se ordinem servent, ut minor quisque majorem praecedat. Primus igitur erit $n \times 3$, secundus $n \times 5$, tertius $n \times 7$, & universim, $n \times m$ cum habiturus est, inter multiplices, locum, quem numerus m in serie.

(q) Ex Cod. MS. vice οὐδαμῶς, quae Wechelii lectio est.

(r) Conjectura pro ἑαυτῶν.

(s) Particulam μὲν ex Cod. MS restitui.

 δεύτεροι

δεύτεροι καὶ σύνθετοι. Τὸ δὲ τρίτον μέρος, τὸ κοινὸν ἀμφοτέρων, ὃ καθ' ἑαυτὸ μὲν δεύτερον καὶ σύνθετον, πρὸς ἄλλο δὲ πρῶτον καὶ ἀσύνθετον, ἐστὶν ἀποτελούμενον ἀριθμοί, κατὰ τὴν ἑαυτῶν ποσότητα πρῶτοι καὶ ἀσύνθετοι μετρούμενοι τινὲς, εἴτις [τούτῳ τῷ τρόπῳ] (t) φαινόμενος, συγκρινόμενος πρὸς ἄλλον ὁμοίως τὴν φύσιν ἔχοντα, ὥσπερ ὁ θ, φαίνεται γὰρ ἐκ τοῦ γ̄ (u) κατὰ τὴν ἰδίαν ποσότητα μετρούμενος· τρὶς γάρ· εἰ συγκρινόμενος πρὸς τὸν κ΄, φαίνεται γὰρ καὶ οὗτος (w) ἐκ τοῦ ε̄, κατὰ τὴν ἑαυτοῦ ποσότητα μετρούμενος· τετράκις γάρ· κοινὸν μέτρον τούτοις οὐκ ἔστιν, εἰ μὴ μόνη ἡ Μονάς.

(t) Voces τούτῳ τῷ τρόπῳ conjecturâ supplevi.

(u) Literam numeralem γ̄ pro voce τρία quæ apud Wechelium legitur, ex Cod. MS restitui.

(w) Voces γὰρ καὶ οὗτος ex Cod. MS. restitui.

Ex

Ex Arithmeticâ Boethii.

Lib. I. c. xvii.

GENERATIO autem ipſorum atque ortus hujuſmodi inveſtigatione colligitur, quam ſcilicet Eratoſthenes Cribrum nominabat; quod cunctis imparibus in medio collocatis, per eam, quam tradituri ſumus, artem, qui primi, quive ſecundi, quique tertii generis videantur eſſe diſtinguitur. Diſponantur enim a ternario numero cuncti in ordinem impares, in quamlibet longiſſimam porrectionem 3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29. 31. 33. 35. 37. 39. 41. 43. 45. 47. 49. His igitur ita diſpoſitis, conſiderandum, primus numerus quem eorum, qui ſunt in ordine poſiti, primum metiri poſſit: ſed, duobus præteritis, illum, qui poſt eos eſt poſitus, mox metitur: et, ſi poſt eundem ipſum quem menſus eſt, alii duo tranſmiſſi ſunt, illum, qui poſt duos eſt, rurſus metitur: et, eodem modo ſi duos quis reliquerit, poſt eos qui eſt, a primo numero metiendus eſt; eodemque modo, relictis ſemper duobus, a primo, in infinitum pergentes metientur. Sed id non vulgo neque confuſe. Nam primus numerus illum, qui eſt poſt duos ſecundum ſe locatos, per ſuam quantitatem metitur: ternarius enim numerus ter * 9 metitur. Si autem poſt novenarium duos reliquero, qui mihi poſt illos incurre-

* Conjecturâ pro *tertia*.

rit, a primo metiendus eſt, per ſecundi imparis quantitatem; id eſt, per quinarium: nam ſi poſt 9 duos relinquam, id eſt 11 & 13, ternarius numerus 15 metietur, per ſecundi numeri quantitatem, id eſt, per quinarii; quoniam numerus ternarius 15 quinquies metitur. Rurſus, ſi a quindenario inchoans duos intermiſero, qui poſterior poſitus eſt, ejus primus numerus menſura eſt, per tertii imparis pluralitatem: nam ſi poſt 15 intermiſero 17 & 19, incurrit 21, quem ternarius numerus ſecundum ſeptenarium metitur; 21 enim numeri ternarius ſeptima pars eſt: atque hoc in infinitum faciens, reperio primum numerum, ſi binos intermiſero, omnes ſequentes poſt ſe metiri, ſecundum quantitatem poſitorum ordine imparium numerorum. Si vero quinarius numerus, qui in ſecundo loco eſt conſtitutus, velit[b] quis, cujus prima ac deinceps ſit menſura, invenire, tranſmiſſis quatuor imparibus, quintus ei quem metiri poſſit, occurrit. Intermittantur enim quatuor impares, id eſt, 7 & 9, & 11 & 13, poſt hos eſt quintus decimus quem quinarius metitur, ſecundum primi ſcilicet quantitatem, id eſt, ternarii; quinque enim 15 ter metiuntur: ac deinceps, ſi quatuor intermittat, eum qui poſt illos locatus eſt, ſecundus, id eſt, quinarius, ſui quantitate metitur: nam poſt quindecim intermiſſis 17 & 19, & 21 & 23, poſt eos 25 reperio, quos quinarius ſcilicet numerus ſuâ pluralitate metitur; quinquies enim quinario multiplicato, 25 ſuccreſcunt; ſi vero poſt hunc quilibet quatuor intermittat, eâdem ordinis ſervatâ

[b] Conjecturâ pro *vel*.
[a] Conjecturâ pro *tertia*.

conſtantiâ,

constantiâ, qui eos sequitur, secundum tertii, id est, septenarii numeri summam, a quinario metitur: atque hæc est infinita processio. Si vero tertius numerus quem metiri possit exquiritur, sex in medio relinquentur; & quem septimum ordo monstraverit, hic per primi numeri, id est, ternarii quantitatem metiendus est: et post illum, sex aliis interpositis, quem post eos numeri series dabit, per quinarium, id est, per secundum, tertii cum mensura percurret: si vero alios rursus sex in medio quis relinquat, ille, qui sequitur, per septenarium ab eodem septenario metiendus est; id est, per tertii quantitatem; atque hic usque in extremum ratus ordo progreditur. Suscipient ergo metiendi vicissitudinem, quemadmodum sunt in ordine naturaliter impares constituti: metientur autem, si per pares numeros, a binario inchoantes, positos inter se impares, ratâ intermissione, transiliant; ut primus duos, secundus quatuor, tertius sex, quartus octo, quintus decem[d]: vel si locos suos conduplicent, & secundum duplicationem terminos intermittant; ut ternarius, qui primus est numerus, & Unus, omnis enim primus Unus est, bis locum suum multiplicet, faciatque bis unum; qui cum duo sint, primus duos medios transeat. Rursus secundus, id est, quinarius, si locum suum multiplicet, 4 explicabitur: hic quoque quatuor [e] intermittat. Item si septenarius, qui tertius est, locum suum duplicet, sex creabit; bis enim 3 senarium jungunt: hic ergo in ordine [f] sex relinquat. Quartus quoque, si locum

[d] Conjecturâ restitui pro 12.

[e] Conjecturâ pro 4.

[f] Conjecturâ pro *ordinem*.

suum duplicet, 8 succrescent; ille quoque octo transiliat: atque hoc quidem in cæteris perspiciendum. Modum autem mensionis, secundum ordinem collocatorum, ipsa series dabit. Nam primus primum quem numerat, secundum primum numerat [g], id est, secundum se; & secundum primus quem numerat, per secundum numerat [g], & tertium per tertium, & quartum item per quartum. Cum autem secundus mensionem [h] susceperit, primum quem numerat secundum primum metitur; secundum vero quem numerat per se, id est, per secundum; & tertium per tertium: & in cæteris eâdem similitudine mensura constabit. Illos [i] ergo si respicias, vel qui alios mensi sunt, vel qui ipsi ab aliis metiuntur, invenies omnium simul communem mensuram esse non posse, neque ut omnes quemquam alium simul numerent; quosdam autem ex his ab alio posse metiri, ita ut ab uno tantum numerentur [k]; alios vero, ut etiam a pluribus; quosdem autem, ut præter Unitatem eorum nulla mensura sit. Qui ergo nullam mensuram præter Unitatem recipiunt, hos Primos & Incom-

[f] Conjecturâ pro 8.

[g] Pro *numerat* mallem in utroque loco, *metitur*, ut aliud sit *numerare*, aliud *metiri*, & sensus sit, "That which the first "number [of the Series] *counts* the first [of its multiples], it "*measures* by the first [of the Series], i. e. by itself. That "which it *counts* the second [of its multiples], it *measures* by "the second [number in the Series]." Sic enim infra legimus de Numero ordine secundo, "primum quem *numerat* secundum "primum *metitur*."

[h] Conjecturâ, pro *mansionem*.

[i] Conjectura, pro *alios*.

[k] Ang. "But so as to be counted in among the multiples of "one number only."

positos

positos judicamus; qui vero aliquam mensuram præter Unitatem, vel alienigenæ partis vocabulum sortiuntur, eos pronunciemus Secundos atque Compositos. Tertium vero illud genus, per se Secundi & Compositi, Primi vero & Incompositi ad alterutrum comparati, hâc inquisitor ratione reperiet. Si enim quoslibet primos [l] numeros, secundum suum in semetipsos multiplices quantitatem, qui procreantur, ad alterutrum comparati, nullâ mensurâ communione junguntur: 3 [m] enim & 5, si multiplices, 3 ter [n] 9 faciunt, & quinquies 5 reddunt 25. His igitur nulla est cognatio communis mensuræ. Rursus 5 & 7 quos procreant, si compares, hi quoque incommensurabiles erunt: quinquies enim 5, ut dictum est, 25, septies 7 faciunt 49; quorum mensura nulla communis est, nisi forte omnium horum procreatrix & mater Unitas [o].

[l] Conjectura pro *illos*.
[m] Conjecturâ, pro *tres*. [n] Conjecturâ pro *tres tertio*.
[o] Sed cave credas, Lector, numeros inter se primos nullos dari præter Primorum Quadratos.

XXIII. *A Letter from Mr.* Christopher Gullet *to* Matthew Maty, *M. D. Sec. R. S. on the Effects of Elder, in preserving Growing Plants from Insects and Flies.*

Tavistock (Devon) August 11, 1771.

S I R,

Read May 14, 1772.

I SHOULD not presume to trouble you as a member of the Royal Society with the following letter, did not the subject seem to promise to be of great public utility. It relates to the effects of Elder;

Sambucus fructu in umbella nigro.

1st. In preserving cabbage plants from being eaten or damaged by caterpillers.

2d. In preventing blights, and their effects on fruit and other trees.

3d. In the preservation of crops of wheat from the yellows, and other destructive insects.

4th. Also in saving crops of turnips from the fly, &c. &c.

1st, I was led to my first experiments, by considering how disagreeable and offensive to our olfactory nerves the effluvia emitted by a brush of green elder

elder leaves are, and from thence, reaſoning how much more ſo they muſt be to thoſe of a butterfly, whom I conſidered as being as much ſuperior to us in delicacy as inferior in ſize. Accordingly I took ſome twigs of young elder, and with them whipt the cabbage plants well, but ſo gently as not to hurt them, juſt as the butterflies firſt appeared; from which time, for theſe two ſummers, though the butterflies would hover and flutter round them like gnomes or ſylphs, yet I could never ſee one pitch, nor was there I believe a ſingle catterpiller blown, after the plants were ſo whipt; though an adjoining bed was infeſted as uſual.

2d. Reflecting on the effects abovementioned, and conſidering blights as chiefly and generally occaſioned by ſmall flies, and minute inſects, whoſe organs are proportionably finer than the former, I whipt the limbs of a wall plumb tree, as high as I could reach; the leaves of which were preſerved green, flouriſhing, and unhurt, while thoſe not ſix inches higher, and from thence upwards, were blighted, ſhrivelled up, and full of worms. Some of theſe laſt I afterwards reſtored by whipping with, and tying up, elder among them. It muſt be noted, that, this tree was in full bloſſom at the time of whipping, which was much too late, as it ſhould have been done once or twice before the bloſſom appeared. But I conclude from the whole, that if an infuſion of elder was made in a tub of water, ſo that the water might be ſtrongly impregnated therewith, and then ſprinkled over the tree, by a hand engine, once every week or fortnight, it would effectually anſwer.

answer every purpose that could be wished, without any possible risk of hurting the blossoms or fruit.

3d. What the farmers call the yellows in wheat, and which they consider as a kind of mildew, is in fact, as I have no doubt but you well know, occasioned by a small yellow fly with blue wings, about the size of a gnat. This blows in the ear of the corn, and produces a worm, almost invisible to the naked eye; but being seen through a pocket microscope, it appears a large yellow maggot of the colour and gloss of amber, and is so prolific that I last week distinctly counted 41 living yellow maggots or insects, in the husk of one single grain of wheat, a number sufficient to eat up and destroy the corn in a whole ear. I intended to have tryed the following experiment sooner; but the dry hot weather bringing on the corn faster than was expected, it was got and getting into fine blossoms ere I had an opportunity of ordering as I did; but however the next morning at daybreak, two servants took two bushes of elder, and went one on each side of the ridge from end to end, and so back again, drawing the elder over the ears of corn of such fields as were not too far advanced in blossoming. I conceived, that the disagreeable effluvia of the elder would effectually prevent those flies from pitching their tents in so noxious a situation; nor was I disappointed, for I am firmly persuaded that no flies pitched or blowed on the corn after it had been so struck. But I had the mortification of observing the flies (the evening before it was struck) already on the corn (six, seven or eight, on a single ear) so that what damage hath accrued, was done before

before the operation took place; for, on examining it last week, I found the corn which had been ſtruck pretty free of the yellows, very much more ſo than what was not ſtruck. I have, therefore, no doubt but that, had the operation been performed ſooner, the corn would have remained totally clear and untouched. If ſo, ſimple as the proceſs is, I flatter myſelf, it bids fair to preſerve fine crops of corn from deſtruction, as the ſmall inſects are the crops greateſt enemy. One of thoſe yellow flies laid at leaſt eight or ten eggs of an oblong ſhape on my thumb, only while carrying by the wing acroſs three or four ridges, as appeared on viewing it with a pocket microſcope.

4th. Crops of turnips are frequently deſtroyed, when young, by being bitten by ſome inſects, either flies or fleas; this I flatter myſelf may be effectually prevented, by having an elder buſh ſpread ſo as to cover about the breadth of a ridge, and drawn once forward and backward by a man over the young turnips. I am confirmed in this idea, by having ſtruck an elder buſh over a bed of young collyflower plants, which had begun to be bitten, and would otherwiſe have been deſtroyed by thoſe inſects; but after that operation it remained untouched.

In ſupport of my opinion, I beg leave to mention the following fact from very credible information, that about eight or nine years ago this county was ſo infeſted with cock-chaffers or oakwebs, that in many pariſhes they eat every green thing, but elder; nor left a green leaf untouched beſides elder buſhes, which alone remained green and unhurt, amid the general devaſtation of ſo voracious a multitude. On reflecting

reflecting on these several circumstances, a thought suggested itself to me, whether an elder, now esteemed noxious and offensive, may not be one day seen planted with, and entwisting its branches among, fruit trees, in order to preserve the fruit from destruction of insects: and whether the same means which produced these several effects, may not be extended to a great variety of other cases, in the preservation of the vegetable kingdom.

The dwarf elder (*ebulus*) I apprehend emits more offensive effluvia than common elder, therefore must be preferable to it in the several experiments.

On mentioning lately to Sir Richard W. Bampfylde, one of the representatives of this county, my observations on the corn crops, and the effects of the elder, &c. he persuaded me to publish them, which in some measure determined my taking this step, of transmitting them to a Society incorporated for promoting the knowledge of natural things, and useful experiments, in which they have so happily and amply succeded, to the unspeakable advantage and improvement both of the old and new world. I have the honour to subscribe myself,

SIR,

Your most obedient,

humble Servant,

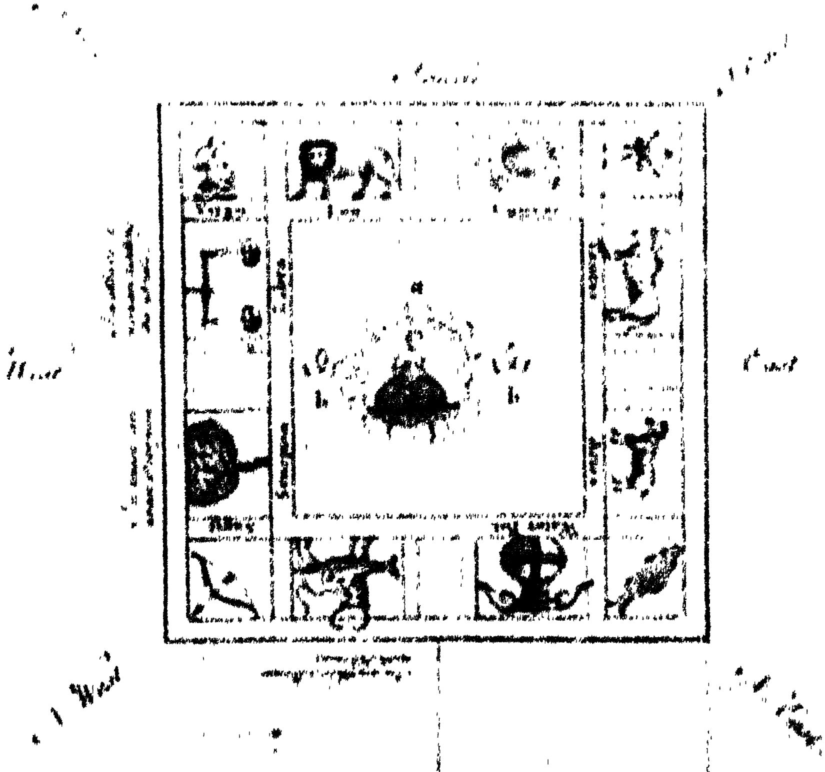
Chr. Gullett.

XXIV. *A*

[illegible]

VERDAPETTAH [illegible] MADURAH COUNTRY

[illegible]



a. Symbols of the Universal Deity.

b.b. Two hooks of Iron to suspend a kind of [illegible] on which the Deity or its [illegible] exhibited to the above.

XXIV. *A Letter from* John Call, *Esq; to* Nevil Maſkelyne, *F. R. S. Aſtronomer Royal, containing a Sketch of the Signs of the Zodiac, found in a Pagoda, near Cape* Comorin *in* India.

S I R,

Read May 14, 1772.

AS a member of the Royal Society, and one whoſe ſtudy is particularly directed to the motions of the heavenly bodies, I think you the moſt proper perſon to whom I can ſend the incloſed ſketch [Tab. X.], which I drew with a pencil, as I lay on my back reſting myſelf during the heat of the day, in a journey from Madurah to Twinwelly, near Cape Comorin. And I ſend it to you rather in the original, as I then ſketched it off, than in any more complete form, leſt it ſhould thereby have more the appearance of compoſition, and leave not ſo ſtrong an impreſſion of antiquity, as it made on me when I diſcovered it.

After ſuch a diſcovery, I ſearched in my travels many other pagodas, or choultrys, for ſimilar carvings; but, to the beſt of my remembrance, never found

 but

but one more equally complete, which was on the ceiling of a temple, in the middle of a tank before the pagoda of Teppecolum, near Mindurah, of which tank and temple Mr. Ward, painter in Broad-ſtreet, near Carnaby-market, hath a drawing; but I have often met with the ſeveral parts in detached pieces.

From the correſpondence of the ſigns of the zodiac which we at preſent uſe, and which we had, I believe, from the Arabians or Egyptians, I am apt to think that they originally came from India, and were in uſe among the Bramins, when Zoroaſter and Pythagoras travelled thither, and conſequently adopted and uſed by thoſe travellers: and as theſe philoſophers are ſtill ſpoken of in India, under the names of Zerdhurſt and Pyttagore, I ſhould alſo hazard another idea, that the worſhip of the cow, which ſtill prevails in India, was tranſplanted from thence to Egypt. But this is only conjecture; and it may with almoſt equal probability be ſaid, that Zoroaſter or Pythagoras carried that worſhip to India.

However, I think there is an argument ſtill in favour of India for its antiquity, in point of civilization and cultivation of the arts and ſciences; for it is hardly in diſpute that all theſe improvements came from the eaſt to the weſt; and, if we may be allowed to draw any concluſions from the immenſe buildings now exiſting, and from the little of the inſcriptions, which can be interpreted on ſeveral of the choultrys and pagodas, I think it may ſafely be pronounced, that no part of the world has more marks of antiquity for arts, ſciences, and civiliza-

tion,

tion, than the peninſula of India, from the Ganges to Cape Comorin; nor is there in the world a finer climate, or face of the country, nor a ſpot better inhabited, or filled with towns, temples, and villages, than this ſpace is throughout, if China and parts of Europe are excepted.

I think the carvings on ſome of the pagodas and choultrys, as well as the grandeur of the work, exceeds any thing executed now-a-days, not only for the delicacy of the chiſſel, but the expence of conſtruction, conſidering, in many inſtances, to what diſtances the component parts were carried, and to what heights raiſed. If Mr. Kittle the painter, now in India, ſhould have time and opportunity, after he hath made his fortune by portrait drawing, it would be a great addition to his reputation, and well worth his pains, to inveſtigate the nature of the Indian architecture and carving, by painting ſome of the moſt curious buildings, or parts of pagodas. The great obſtacle to aſcertaining dates, or hiſtorical events, is the loſs of the Sans-Skirrit language, and the confinement of it to the prieſthood. I ſhould have taken ſome pains to have collected many things; but the number of revolutions and occupations which happened always prevented me.

I alſo commit to your inſpection the * manuſcripts of Mr. Robins, which he gave me at his death;

* Theſe I communicated to the Royal Society, together with this letter; but being examined by myſelf, Mr. Raper, Mr. Cavendiſh, and Mr. Horſley, at the deſire of the Society, they were not found to contain any thing material more than has been already printed; excepting a treatiſe on military diſcipline: which, if it ſhould be thought of uſe, may be inſerted in the next edition of his works. N. M.

 I be-

I believe moſt of them have been printed, but if there are any which have not, or that can amuſe you or inſtruct others, you are welcome to uſe them as you pleaſe: I only wiſh they may contain any thing uſeful. While he lived, I purſued thoſe ſtudies; but, ſoon after his death, new ſcenes aroſe, and engaged me more in practical ſervice, than allowed me time for theory, or experiments. I am, however, a conſtant well-wiſher to the progreſs of arts and ſciences, as well as ſtudy; and very much,

SIR,

Your obedient,

humble ſervant,

Jn° Call.

XXV. *An Account of the Flowing of the Tides in the* South Sea, *as observed on board His Majesty's Bark the* Endeavour, *by Lieut.* J. Cook, *Commander, in a Letter to* Nevil Maskelyne, *Astronomer Royal, and F. R. S.*

Mile-end, February 5, 1772.

Reverend Sir,

Read May 21, 1772.

I Here send you the few observations I made on the tides in the South Sea, to which I have only to add, that, from many circumstances and observations, I am fully convinced that the flood comes from the southward, or rather from the S. E. I am,

S I R,

Your most obedient,

humble servant,

J. Cook.

Names

Names of places where observed.	Lat. South.		Long. West.		New and full Moon. High water.		Rise & fall.	
					H.	M.	F.	In.
Success Bay in Strait le Maire —	54	45	66	4	4	30	5	0
Lagoon Island — — —	18	47	139	28	0	30	. .	. .
Matavai Bay, Otaheite — —	17	29	149	30	0	30	0	11
Tolaga Bay, East coast of New Zealand	38	22	181	14	6	0	5	6
Mercury Bay, N. E. ditto — —	36	48	184	4	7	30	7	0
River Thames, ditto — — —	37	12	184	12	9	0	10	0
Bay of Islands, ditto — — —	35	14	185	36	8	0	7	0
Queen Charlotte's Sound, Cook's Strait New Zealand — — —	41	0	184	45	9	30	7	6
Admiralty Bay, in ditto — —	41	45	185	11	10	0	7	0
Botany Bay, coast of New South-Wales	34	0	208	37	8	0	4	6
Bustard Bay, ditto — — —	24	30	208	30	8	0	8	0
Thirsty Sound, ditto — — —	25	5	210	24	11	0	16	0
Endeavour River, ditto — —	15	26	214	48	9	30	9	0
Endeavour's Strait, which divides New Guinea from New Holland —	10	37	218	45	1	30	11	0

XXVI. An

XXVI. *An Account of a new Electrometer, contrived by Mr.* William Henly, *and of several Electrical Experiments made by him, in a Letter from Dr.* Prieſtley, *F.R.S.* *to Dr.* Franklin, *F. R. S.*

Dear Sir,

Read May 28, 1772.

I THINK myſelf happy in an opportunity of giving you a ſpecies of pleaſure, which I know is peculiarly grateful to you as the father of modern electricity, by tranſmitting to you an account of ſome very curious and valuable improvements in your favourite ſcience. The author of them is Mr. Henly, in the Borough, who has favoured me with the communication of them, and has given me leave to requeſt, that you would preſent them to the Royal Society.

In my hiſtory of electricity, and elſewhere, I have mentioned a good electrometer, as one of the greateſt deſiderata among practical electricians, to meaſure both the preciſe degree of the electrification of any body, and alſo the exact quantity of a charge before the exploſion, with reſpect to the ſize of the electrified body, or the jar or battery with which it is connected; as well as to aſcertain the moment of time, in which the electricity of a jar changes, when, without making an exploſion, it is diſcharged by giving

giving it a quantity of the contrary electricity. All these purposes are answered, in the most complete manner, by an electrometer of this gentleman's contrivance, a drawing of which I send you along with the following description.

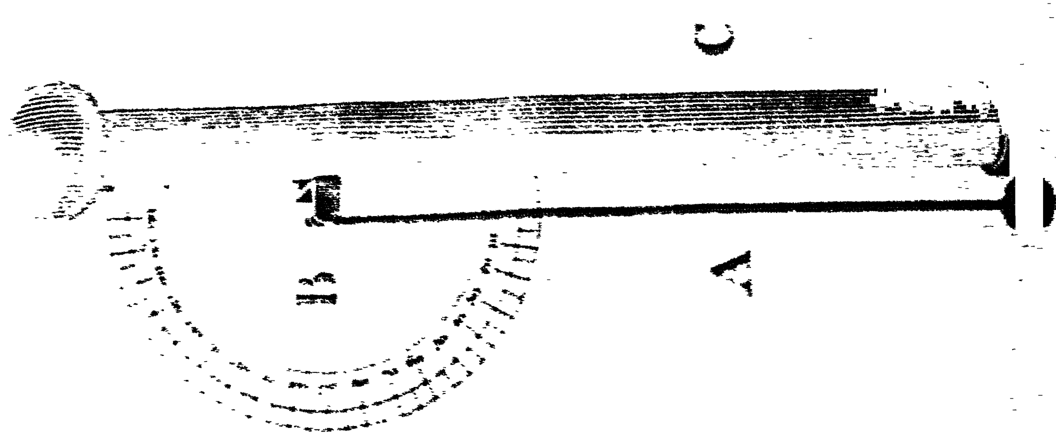
The whole instrument is made of ivory or wood. [Tab. XI.] (*a*) is an exceeding light rod, with a cork ball at the extremity, made to turn upon the center of a semicircle (*b*), and so as always to keep pretty near the limb of it, which is graduated: (*c*) is the stem that supports it, and may either be fixed to the prime conductor, or be let into the brass knob of a jar or battery, or set in a stand, to support itself.

The moment that this little apparatus is electrified, the rod (*a*) is repelled by the stem (*c*), and consequently begins to move along the graduated edge of the semicircle (*b*); so as to mark with the utmost exactness, the degree in which the prime conductor, &c. is electrified, or the height to which the charge of any jar or battery is advanced; and as the materials of which this little instrument is made are very imperfect conductors, it will continue in contact with any electrified body, or charged jar, without dissipating any of the electricity.

If it should be found, by trial in the dark, that any part of this instrument contributes to the dissipation of the electric matter, (which, when the electrification was very strong, I once observed mine to do) it should be baked * a little, which will presently prevent it. If it is heated too much, it will not receive electricity readily enough; and then the motion of the index will not correspond with sufficient

* Warmed a little, to dry off the damps, particularly from the index.

exactness,



The Electrometer is found by experience to be the most perfect, when the stem and the index are of Box, made very smooth with Emery paper. The ball should be Cork, the graduated plate Ivory, as the divisions on that substance are more legible than on wood.

exactneſs, to the degree in which the body to which it is connected is electrified; but this inconvenience is eaſily remedied, by moiſtening the ſtem and the index, for the ſemicircle cannot be too dry.

I find by experience, that this electrometer anſwers all the purpoſes I have mentioned, with the greateſt eaſe and exactneſs. I am now ſure of the force of any exploſion before a diſcharge of a jar or battery, which I had no better method of gueſſing at before, than by preſenting to them a pair of Mr. Canton's balls, and obſerving their divergency at a given diſtance; but the degree of divergency was ſtill to be gueſſed at by the eye, and the balls can only be applied occaſionally; whereas this inſtrument, being conſtantly fixed to the prime conductor or the battery, ſhews, without any trouble, the whole progreſs of the charge; and, remaining in the ſame ſituation, the force of different exploſions may be aſcertained with the utmoſt exactneſs before the diſcharge.

If a jar be loaded with poſitive electricity, and I want to know the exact time when, by attempting to charge it negatively, it firſt becomes diſcharged, I ſee every ſtep of its approach to this ſtate by the falling of the index; and the moment I want to ſeize, is the time when it has got into a perpendicular ſituation, which may be obſerved, without the leaſt danger of a miſtake. Accordingly I find that, in this caſe, not the leaſt ſpark is left in the jar. If I continue the operation, the index, after having gained its perpendicular poſition, begins to advance again, and thereby ſhews me the exact quantity of the oppoſite electricity that it has acquired.

 Conſi-

Conſidering the admirable ſimplicity, as well as the great uſefulneſs of this inſtrument, it is ſomething ſurprizing that the conſtruction ſhould not have occurred to ſome electrician before this time. Nollet's and Mr. Waits's invention of threads, projecting ſhadows upon a graduated board, reſembled this apparatus of Mr. Henly's, but was a poor and awkward contrivance in compariſon with it; nor was Richman's gnomon, though a nearer approach to this conſtruction, at all comparable to it; and the ingenious author of it had no knowledge of either of thoſe methods when he hit upon this.

I have made a receptacle for this inſtrument in my prime conductor, and I have alſo a pedeſtal in which I can fix it; and by means of which I can very conveniently place it on the wires of a battery.

In either of thoſe ſituations it anſwers almoſt every purpoſe of an electrometer, without removing it from its place.

I doubt not that you and all other electricians will join with me in returning our hearty thanks to Mr, Henly for this excellent and uſeful inſtrument.

Many of the effects of my battery, in breaking of glaſs, and tearing the ſurface of bodies, Mr. Henly performs by a ſingle jar, only increaſing the weight with which the bodies are preſſed, while the exploſion is made to paſs cloſe under them.

By this means he raiſes exceeding great * weights, and ſhatters ſtrong pieces of glaſs into thouſands of the ſmalleſt fragments; he even reduces thick plate glaſs by this means to an impalpable powder. But

* Frequently ſix pounds Troy.

what

what is moſt remarkable is, that when the pieces of glaſs are thick, and ſtrong enough to reſiſt the ſhock, they are marked by the exploſion, with the moſt lively and beautiful colours, generally covering the ſpace of about an inch in length, and half an inch in breadth.

In ſome of the pieces which he was ſo obliging as to ſend me, theſe colours lie all intermixed and confuſed; but in others I obſerve them to be diſpoſed in priſmatic order, in lines parallel to the courſe of the exploſion, and in ſome (as N° 1.) I have counted three or four diſtinct returns of the ſame colour.

He has lately informed me, that, ſince he ſent me this piece, he has ſtruck theſe priſmatic colours into another maſs of glaſs, in a ſtill more vivid and beautiful manner, the colours ſhooting into one another. This effect, he ſays, was produced by making a ſecond exploſion, without moving any of the apparatus after the firſt.

When the glaſs in which theſe colours are fixed is examined, it is evident that the ſurface is ſhattered into thin plates, and that theſe give the colours, the thickneſs of them varying regularly, as they recede from the path of the exploſion.

In the middle of theſe coloured ſpots (as in N° 2.) ſome of theſe thin plates, or ſcales, are ſtruck off, I ſuppoſe by the force of the exploſion; and with the edge of a knife they are all eaſily ſcraped away, when the ſurface of the glaſs is left without its poliſh (as in N° 3.)

The piece of glaſs on which I have marked theſe numbers, as well as that on which he has ſtruck the

 colours

colours in a ſtill more beautiful manner, Mr. Henly will preſent to the Royal Society, for the inſpection of the members.

Beſides theſe improvements, Mr. Henly has likewiſe, in a very ingenious manner, diverſified ſeveral of the more entertaining experiments in electricity, particularly in his imitation of the effects of earthquakes by the lateral force of exploſions; and he has alſo hit upon ſeveral curious facts, that, unknown to him, had been obſerved before by others: the following particular, however, I believe is new, exciting a ſtick of ſealing wax, and uſing a piece of tin foil for the rubber, he found that it would electriſy poſitively, as well as glaſs rubbed with ſilk and amalgama.

Wiſhing we had more ſuch fellow labourers as Mr. Henly, I am,

DEAR SIR,

Your obliged

humble ſervant,

Leeds,
Oct. 26, 1770.

J. Prieſtley.

XXVII. *Me-*

Read May 28, 1772.

XXVII. *Meteorological Obſervations at* Ludgvan *in* Mount's-Bay, Cornwall, 1771: *By* William Borlaſe*, *D. D. F. R. S. Communicated by Dr.* Jeremiah Milles, *Dean of* Exeter, *and F. R. S.*

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
January	Higheſt 23 30,5 Loweſt 19 29,0	The 1ſt at night a violent ſtorm, and rain till midnight. On the 2d at 8 P. M. a violent ſtorm, which continued all night; ſtormy the 3d and 4th; ditto the 26th at night; wind Weſterly. On the 10th at night, after hail ſhowers, a great fall of ſnow; the 11th great ſnow falling, with ſtormy blaſts; the 12th deep ſnow and more falling, with froſt; deep ſnow, and hard froſt, the 13th, 14th, 15th, 16th, 17th, 18th, and 19th, ſnow lying deep, but the froſt more gentle and the thaw came on; the 20th P. M. it thawed faſt; on the 21ſt in the afternoon, the froſt and ſnow was all gone; the reſt of the month moſtly miſts with ſome hard ſhowers of rain. Wind, during the ſtorms, miſts, and rain, Weſterly for 16 days; during the cold, Eaſt, and Eaſt North Eaſt.	Monthly Med. of heat for each day. Higheſt 1 50 Loweſt 17 $27\frac{1}{2}$ } $39\frac{5}{31}$	Inches. $3\frac{300}{1000}$

* This is the laſt paper of this kind, which the Society will receive from the excellent author of the Natural Hiſtory of Cornwall, and ſeveral other learned works; death having, though at an advanced age, put a period to a life divided between the purſuit of uſeful and experimental knowledge, and the faithful diſcharge of every moral and religious duty. M. M.

Month

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
			Med.	inches.
February	Highest 3 30,16 Lowest 25 28,87	Calm, the 3d, 4th, 5th, 6th, 7th, 9th, 14th, 15th, 17th, 18th, 21st, 22d; hard frost with some snow on the 9th, 10th, 11th, 12th, 13th, 14th. It then thawed, and the rest was hazy, misty, showery, with some high winds on the 15th and 27th. Wind, during the cold, East and North, the rest South for 18 days.	Highest 21 52 Lowest 11 33 } [illegible]	1,500
March	Highest 19 30,6 Lowest 12 29,15	Calm, the 3d, 4th, 5th, 6th, 7th, 8th, 15th, 18th, 19th, 20th, 21st, 27th, 31st. Frost 6th, 7th, 23d, 25th, 28th; hail, snow, and sleet 23d, 24th, 25th, 27th, 28th. Stormy the 1st, 13th. Wind 27 days from the East mixed equally with North and South.	Highest 13 [illegible] Lowest 25 [illegible] } 41 [illegible]	2,900
April	Highest 18 30,27 Lowest 30 29,50	Calm, the 1st, 5th, 9th, 10th, 12th, 13th, 14th, 19th, 20th, 21st, 23d, 26th, 27th, 29th, 30th. Hailed, snow lying only 2 days, viz. the 15th and 16th. Rest mostly fair, and dry. Wind 18 days from the East; the rest mixed, and changeable.	Highest 22 53 Lowest 16 35 } 46 [illegible]	2,900
May	Highest 23 30,8 Lowest 7 29,18	Calm, 1st, 2d, 3d, 4th, 5th, 8th, 9th, 10th, 11th, 12th, 13th, 14th, 15th, 16th, 17th, 18th, 19th, 20th, 21st, 22d, 23d, 24th, 25th, in all 23 days. Stormy only on the [illegible]. Wind Southerly 23 days; the rest mixed.	Highest 12 65 [illegible] Lowest 1 45 } 53 [illegible]	2,250

Month.

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
			Med.	Inches.
June	Highest 3 30,15 Lowest 1 29,54	Calm, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 11th, 12th, 13th, 14th, 15th, 20th, 21st, 22d, 23d, 24th, 25th, 26th, 27th, 28th, 29th, 30th (in all 24 days) the wind variable and mixed, but the weather remarkably settled, fair, and pleasant. On the 28th however, there was most violent thunder, lightning, and a flood of rain, at the towns of Penryn and Falmouth, 20 miles distant from Mount's-Bay to the East; but in Mount's-Bay, the air was cloudy, and only some distant thunders; the lightning was scarce visible, and not a drop of rain.	Highest 27 $71\frac{1}{2}$ Lowest 3 49 } $58\frac{2}{30}\frac{1}{3}$	0,200
July	Highest 14 30,15 Lowest 31 29,50	Calm, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 10th, 13th, 14th, 15th, 16th, 17th, 18th, 23d, 24th, 25th, 27th, 28th, 29th, 30th, the rest mixed. Wind 24 days from the West, mixed mostly with the South. N. B. As we had a most unusual run of dry weather here in Cornwall; in other parts of the world, they had altogether as extraordinary a glut of rain. See, for particulars, the publick papers, from Berlin, Dresden, Hamburgh, and Vienna, &c. in Europe, and from Virginia in North America, where their inundations have not been remembered so destructive.	Highest 17 72 Lowest 31 $54\frac{1}{2}$ } $61\frac{5}{31}\frac{1}{4}$	0,720

Month.

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Med.	Ombr.
					Inches.
August	Highest 29 30,0 Lowest 19 29,40	Calm, 1st, 2d, 3d, 4th, 5th, 6th, 15th, 17th, 22d, 23d, 26th, 27th, 28th, 29th, 30th, 31st; the rest mists, fogs, and rains, with some gales interspersed. Wind 28 days from the West, mixed, nearly equal with North and South.	Highest 31 65 Lowest 1 54	58 [illegible]	3,633
September	Highest 28 30,6 Lowest 20 29,30	Calm, 2d, 3d, 5th, 6th, 10th, 11th, 12th, 13th, 14th, 15th, 16th, 17th, 18th, 24th, 25th, 26th, 28th; the rest hazy, cloudy, windy, mixed with rain. Wind mostly West, mixed with the North.	Highest 1 61 Lowest 20 46½	55 [illegible]	3·400
October	Highest 31 30,35 Lowest 13 28,85	Calm, only 8 days A. M. only 2 days P. M.; the rest rainy, windy, stormy. A violent storm on the 13th and 14th; the extream on the 14th at 10 P. M. wind South West. N. B. on the 13th, at Caton near Lancaster, happened the greatest inundation in the memory of man Wind 23 days from the West, mixed mostly with the South.	Highest 8 57 Lowest 31 43	51 [illegible]	4·550
November	Highest 18 30,40 Lowest 11 29,40	Calm, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 13th, 18th, 19th, 20th, 22d, 25th, 26th, 27th. On the 11th, 12th, 15th, 16th, stormy with rain and showers. Wind 24 days from the West, mixed mostly with the South. N. B. This month was very dry in Cornwall; but by the incessant rains in the middle of it, from the 15th to the 17th, such inundations happened at Newcastle, Durham, Barnard-castle, and near Carlisle, by the breaking out of Solway-moss, as have never been known to desc[illegible]ture.	Highest 16 55 Lowest 10 39	[illegible]	1,450

Month.

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.	Ombr.
			Med.	Inches.
December	Highest 30 30,4 Lowest 7 28,40	Calm, 1st, 2d, 8th, 13th, 17th, 18th. On Friday the 6th (new-moon at 8 A. M. wind South by East, and South East) about 8 P. M. it blew a violent storm, and setting full into Mount's-Bay at spring-tide, the sea was so high, and furious, that it demolished houses, cellars, boats, and walls, wherever it reached. The four towns on the shore all suffered; and it has been calculated, that not less than 5000 £. damage was done here that night, besides ships lost. It reached to the Eastward; and at Plymouth, about 60 miles off, they reckoned the tide was higher by ten feet than usual. The remainder of this month was showery, rainy, windy. Wind Westerly 24 days, mixed mostly with the South.	Highest 11 54 Lowest 30 36 } $46\frac{29\frac{3}{4}}{31}$	5,350

The whole Rain fallen in this Year 1771, at Ludgvan, a very dry Year, } $30,\frac{153}{1000}$

XXVIII. *Account*

XXVIII. *Account of ſeveral Quadrupeds from* Hudſon's Bay*, *by Mr.* John Reinhold Forſter, *F. R. S.*

Read May 21, 1772.

1. Arctic Fox, Penn. Synopſ. of Quadr. p. 155. n. 113. *Canis Lagopus*, Linn.

Severn River.

A moſt beautiful ſpecimen in its ſnowy winter furr; this animal ſeems to be lower on its legs than the common fox, and is prodigiouſly well ſecured againſt the intenſe cold of the climate, by the thickneſs and length of its hairs, which are at the ſame time as ſoft as ſilk.

* Among the occaſional advantages, which the obſervations of the laſt Tranſit of Venus have procured, that of receiving uſeful informations from, and ſettling correſpondencies in, ſeveral parts of the world, is not the leaſt conſiderable. From the factory at Hudſon's Bay, the Royal Society were favoured with a large collection of uncommon quadrupeds, birds, fiſhes, &c. together with ſome account of their names, place of abode, manner of life, uſes, by Mr. Graham, a gentleman belonging to the ſettlement on Severn River; and the governors of the Hudſon's Bay Company have moſt obligingly ſent orders, that theſe communications ſhould be from time to time continued. The deſcriptions contained in the following papers were prepared and given by Mr. Forſter, before his departure on an expedition, which will probably open an ample field to the moſt important diſcoveries. M. M.

The

The account ſent along with it from Severn River ſays, that theſe white foxes are ſilly, inoffenſive animals; and are known to ſtand by, whilſt a trap is baited for them, into which they put their heads immediately: they will, when pinched by hunger, devour thoſe of their own kind, which are already caught in theſe traps. But the moſt curious circumſtance is, their migration to the Northward and the Eaſtern coaſts of the bay; for though a few of them are caught every year near York fort and Churchill river, yet, once in three or four years, they come in great numbers; and ſeveral hundred of their furrs are ſent to England in that plentiful ſeaſons, which always begins in November, and ends in April. The ſpecimen ſent is full grown, and its furr quite in ſeaſon.

2. Lesser Otter. Penn. Syn. Quadr. p. 239. n. 174. *Muſtela Lutreola* Linn. Syſt. Nat. 66. Faun. Succ. N° 13.
Severn River.

I am ſtill dubious, whether this animal ought to be looked upon as the ſame with the leſſer otter of Europe and Aſia; many circumſtances ſeem to prove this identity; but ſome, ſuch as the want of webs, which I could not diſcover between the toes, and the white ſpot on the neck, will not admit of it. I have, therefore, ſubjoined a deſcription of this creature at the end of this article. The natives of Hudſon's Bay call this quadruped

Jackash; Mr. Graham from Severn river says, that it harbours about creeks, and lives on fish, like the otter; it travels very slowly, and has from four to seven young at a time; in size it equals the marten; its length is about 16 inches; its whole body is covered with shining dark brown hairs, which lie very close, and seem perfectly convenient for an amphibious animal; under these brown hairs the woolly hairs are tawny, the whole under-jaw is encompassed by a stripe of white hairs, and a little irregular spot of the same colour appears in the middle of the throat; the feet are quite covered with hair to the very nails, which are small, five on each foot, and of a whitish semipellucid colour; the tail is pretty well beset with hair, though not bushy, and much blacker than the rest of the body; it is about half as long as the whole animal.

3. PINE MARTEN. Penn. Syn. Quad. p. 216. n. 155. *Mustela Martes (Abietum).* Linn.
Severn River. Male and Female.

These seem to be a variety of the yellow-breasted marten, Br. Zool. I. 81. their colour, especially in the females, being much paler than that described in Mr. Pennant's works. The male is of a chesnut brown, the female a bright tawny yellow; the former has here some dark brown hairs, the latter in the same manner has some bright bay hairs. They both have white cheeks, and white tips of the ears. Their furrs are very full of hair,

proper

proper to preſerve them from the cold. The tail in both ſexes is buſhy, and darker than the reſt of the body; in the female indeed it is tawny, with a black tip; in both it is ſhorter than deſcribed by Mr. Pennant, Mr. Briſſon, and others, and was perhaps mutilated. This ſpecies feeds on mice, rabbits, &c. though it will not touch a dead mouſe which is put as a bait in a trap, and therefore the inhabitants are obliged to make uſe of a partridge's head, or the like, for that purpoſe. If purſued with noiſe, it immediately gets up into a tree. Some gentlemen have unſucceſsfully attempted to tame theſe creatures, and thoſe kept in cages with that view have been obſerved to be troubled with epileptick fits. Numbers of them are caught at Hudſon's Bay in traps made of ſmall ſticks. They burrow under ground, and bring forth from four to ſeven young at a time.

4. Stoat and Ermine. Penn. Syn. Quad. p. 212. n. 151. α. β. *Muſtela Erminea.* Linn.

Severn River, Albany Fort.

One in the ſummer and another in the winter dreſs. The natives about Albany call them *Sic-cuſe-ſue*, but it is not known why they give them that name. They feed on mice, ſmall birds, all ſort of fiſh, fleſh, and fowl.

5. Common Weesel. Penn. Syn. Quadr. p. 211. n. 150. *Muſtela nivalis.* Linn.

One in its winter dreſs, length 7 inches, tail about 1 inch, perhaps mutilated; it is quite white, but the

the coat is mixed here and there with a brownish hair, especially in the tail. Another in the summer coat, the same as our weesel.

6. SKUNK. Penn. Syn. Quadr. p. 233. n. 167. Kalm's Travels, I. 273. tab. I.

It answers to Mr. Pennant's description, except that the white stripe on the head is not connected with that on the back, and that the brown area, which is left between the two white stripes on the back, is broader than he describes it.

7. CANADA PORCUPINE. Penn. Syn. Quadr. p. 266. n. 196. *Hystrix dorsata.* Linn.

Severn River.

It agrees perfectly with the descriptions. These animals live among the pine trees, of which the bark is their food in winter, as willow tops and the like are in summer. They copulate in September, and bring forth only one young the first week in April. During winter they seldom travel above five hundred yards, so that one is always sure of finding a porcupine, as soon as one meets with a tree that has been fresh stripped of its bark. The longest quills of an old porcupine are about five inches long. The Europeans are very fond of the flesh of these animals, as it tastes, when roasted, exactly like that of a sucking pig. Their bones in winter have a greenish yellow colour, perhaps owing to their continually feeding on the bark of pine trees. It is known that

that the bones of animals will become red by their feeding on madder.

8. BEAVER. Penn. Syn. Quadr. p. 255. n. 190. *Castor Fiber*. Linn.

Churchill River, N° 1.

A most beautiful specimen, in high preservation, and in full season; the furr is of a fine jetty black: the skull of another has likewise been sent. There is a great similarity in the conformation of the cutting teeth of this and the preceding quadruped (the porcupine); only the latter has them longer.

9. MUSK-BEAVER. Penn. Syn. Quadr. p. 259. n. 121. *Castor Zibethicus*. Linn.

Musquash. Severn River.

It frequents the plains, builds a house like the beaver, brings forth from five to seven young at a time, and feeds on poplars, willows, and grass.

10. ALPINE HARE. Penn. Syn. Quadr. p. 249. n. 185. *Lepus timidus*. Linn. Kalm's Trav. into N. Amer. III. p. 59.

York Fort.

A fine specimen, in its compleat winter furr, being quite white, except the ears, which have black tips. It is much larger than the following animal. The common hare, *Penn. Syn. Quadr.* does not seem to be a native of America.

11. AMERICAN HARE, called Rabbit at Hudson's Bay. Kalm's Trav. into N. Amer. I. 105. II. 45. Severn and Churchill Rivers.

This species, which has been improperly called Rabbit, perhaps because it is less than the hare, is certainly new, and was never described before, except by Kalm in his travels through North America, Vol. I. 105. II. 45. The account he there gives corresponds with that of Mr. Graham, and with the specimen now in the Royal Society's collection. These animals are numerous at Hudson's Bay; they do not burrow under ground, but live summer and winter under windfalls and roots of trees. They do not migrate, but always keep about the same place, unless disturbed. They breed once or twice a year, and have five to seven young at a time: their weight is from 3 to 4½ pounds. Their flesh is not so white and delicate as that of the common rabbit, but yet is good food in summer and winter. Great numbers of them are annually caught in the following manner; as they always are used to go one particular path, the English and natives lay young trees across it, forming a hedge, in which there is an opening for the creature to go through; in this place they fix a snare, made of brass wire, packthread, or the like, fastened with a slipping knot to a cross piece, the end being tied to an elastic pole; so that when the animal puts its head into

into the ſnare, the knot is drawn from the croſs piece above, and the pole flying up, immediately ſuſpends the animal in the air.

The proper characteriſticks of this ſpecies ſeem to be,

1. Its ſize, which is ſomewhat bigger than a rabbit's, but leſs then that of the Alpine or leſſer hare.
2. The proportion of its limbs, its hind feet being longer in proportion to the body than thoſe of the rabbit and the common hare. Vide the Hon. Daines Barrington's, V.P.R.S. letter to Dr. Watſon on this new ſpecies of hare, in this volume, p. 6.
3. The tips of the ears and tail, which are conſtantly grey not black. Kalm's Trav. II, p. 45.

Perhaps ſome other characters might be aſcertained, if the animal was brought over in its perfect ſummer furr; for all the ſpecimens in the Royal Society's Muſeum are either entirely in their winter dreſs, or in a changing condition. Mr. Kalm mentions, that thoſe which are found in New Jerſey, where the climate is much more mild than at Hudſon's Bay, keep the ſame grey colour both ſummer and winter; that in ſpring they breed in hollow trees, but in ſummer in the graſs; that, when purſued, they immediately take refuge in hollow trees, whence they are driven out by crooked ſticks, ſmoak, &c.; laſtly, that they do much miſchief to cabbage fields and orchards, by eating the cabbage plants, and

 the

the bark of the apple trees, feeding only by night, as the common hare.

12. QUEBEC MARMOT; Penn. Syn. Quadr. p. 270. n. 199.

Churchill River, N° 5.

This creature is called a ground squirrel, at Churchill fort; it differs much in size from that described in the Syn. Quadr. being much less than a rabbit, perhaps it is a young one. I took down the following description, as I did not find it exactly corresponding with that of the Canada marmot. The nose is blunt, the ears are short and roundish, the top of the head chesnut, back all over sprinkled with whitish, black, and yellowish brown: the legs and whole underside of the animal are of a bright ferruginons colour; the tail is very short, and black at the tip. The length of the animal from the nose to the beginning of the tail is about 11 inches, that of the tail 3 inches. Its toes on the fore feet 4, hind feet 5.

13. COMMON SQUIRREL. Penn. Syn. Quadr. p. 279. n. 206. *Sciurus vulgaris*, Linn.

A variety of the common species, being somewhat inferior in size, having a ferruginous back and grey belly, a shorter tail than the common European sort, of a fine ferruginous red, edged only with black. This animal lives in pine trees, of which the cones are its food; it lies dormant the greater part of the winter.

14.

14. Greater flying Squirrel.
Severn River.

It is equal in ſize, if not bigger than the common ſquirrel; has pretty long hairs, duſky at bottom, tawny brown at the very tips only; and diſpoſed ſo that the back appears wholly of that reddiſh brown colour; the tail is very buſhy, ſomewhat compreſſed, but not pinnated (i. e. with the hairs diſpoſed horizontally on each ſide of it, as for example in the common ſquirrel), it is browniſh on the upperſide with a duſky tip, of a yellowiſh white below; the whole underſide of the animal has the ſame yellowiſh white colour. The membrane reaches from the forefeet to the hindfeet, without extending to the ears; it is found in James's Bay, about 51° north latitude.

This is perhaps Linneus's *Sciurus volans*, and the ſame with the flying ſquirrel of the Arctick parts of Europe. Mr. Briſſon ſeems to have confounded this, and the little Virginian ſquirrel together, and his quotations are quite confuſed. Linneus's *Mus volans* certainly is a variety of the little flying ſquirrel, of the milder parts of North America, New York, Pennſylvania, Virginia, which is vaſtly different from this in ſize and colour.

15. A small Animal, called a Field Mouſe.
Churchill River.

A ſpecimen in very bad preſervation, wanting legs, tail, &c which makes it impoſſible to de-

termine of what ſpecies it is; its ſize is ſomewhat ſuperior to that of a mouſe, its colour duſky, mixed wirh tawny brown, and dirty white on the belly; its head is broad, like that of the ſhort-tailed field mouſe, and has a duſky line in the middle between the eyes, which extends, though rather indiſtinctly, all along the back; its ears are very ſmall and roundiſh.

16.

This is likewiſe a very bad mutilated ſpecimen, leſs than the common mouſe, duſky and brown above, and whitiſh below; its ears are pretty large and prominent.

17. FIELD MOUSE. Penn. Syn. Quadr. p. 302. n. 230. *Mus Sylvaticus*, Linn.

Two ſpecimens; the deſcriptions anſwer pretty well, the ears are large and round, the tail is very long and whitiſh below.

18. SHORT-TAILED MOUSE. Penn. Syn. Quadr. p. 305. n. 233. *Mus terreſtris*, Linn. Le Campagnol de Buffon.

Mr. Pennant's admeaſurements do not quite anſwer, but M. d'Aubenton's coincide.

19. FOETID SHREW. Penn. Syn. Quadr. p. 307. n. 235. *Sorex Araneus*, Linn.

The ſpecimen is much blacker on the back than the European Shrew, its ſides are reddiſh brown.

20. SHREW; two ſpecimens.

The colour is of a duſky grey above, and a dirty white or yellowiſh below; the noſe is very long and ſlender; the length from the noſe to the tail, in the one ſpecimen is 2¼, in the other almoſt 2 inches; the tail is about an inch and half long, thinly beſet with hairs, brown above, and yellowiſh below. If this ſpecies had no tail, I ſhould take it to be the minute Shrew, which the Rev. Mr. Laxman found in Siberia, and which is the *Sorex minutus.* Linn.

XXIX. *An Account of the Birds sent from* Hudson's Bay; *with Observations relative to their Natural History; and* Latin *Descriptions of some of the most uncommon. By* J. R. Forster, *F. R. S.*

Read June 18—25, 1772.

I. Land-Birds.

1. { Accipitres / Rapacious. Faun. Am. Sept.

1. FALCO, Falcon. } 1. Columbarius. 128. 21. Pigeon Hawk. Faun. Am. Sept. p. 9. Catesby I. t. 3. Epervier de la Caroline. Brisson I. p. 378. Severn river, N° 19.

This species is called a *small-bird hawk* at Hudson's Bay. It is migratory, arriving near Severn River in May, breeding on the coast, and then retiring to a warmer climate in autumn. It feeds on small birds; and, on the approach of any person, will fly in circles, making a hideous shrieking noise. The breast and

and belly are yellowiſh, with brown ſtreaks, which are not mentioned by the ornithologiſts, though their deſcriptions anſwer in other reſpects. It weighs ſix ounces and a half, its length is 10½, the breadth 22½. Cateſby's figure is a very indifferent one.

FALCO, 2. Spadiceus. *New Species.* Chocolate Falcon. Faun. Am. Sept. p. 9.

This ſpecies, at firſt ſight, bears ſome reſemblance to the European Moor Buzzard, or *Aeruginoſus*, Linn. but is much leſs, and wants the light ſpots on the head and ſhoulders. No number or deſcription was ſent along with it.

FALCO, 3. Sacer, Briſſon, I. p. 337. Sacre de Buffon, Oiſeaux, (edition in 12mo.) Tom. II. p. 349. t. 14. Faun. Am. Sept. p. 9.

Severn River; N° 16.

Speckled Partridge Hawk, at Hudſon's Bay. The name is derived from its feeding on the birds of the Grous tribe, commonly called partridges, at Hudſon's Bay. Its irides are yellow, and the legs blue. It comes neareſt the *Sacre* of Briſſon, Buffon, and Belon; but Buffon ſays it has black eyes, which is very indiſtinct; for the irides are black in none of the falcons, and in few other birds; and the pupil, if he means that, is black in all birds. It is ſaid, by Belon, to come from Tartary and Ruſſia, and is, therefore, probably a northern bird. It is very voracious

and

and bold, catching partridges out of a covey, which the Europeans are driving into their neſts. It breeds in April and May. Its young are ready to fly in the middle of June. Its neſts, as thoſe of all other falcons, are built in unfrequented places; therefore, the author of the account from Severn River could not aſcertain how many eggs it lays; however, the Indians told him it commonly lay two. It never migrates, and weighs 2½ pounds; its length is 22 inches, its breadth 3 feet.

2. STRIX. 14. Brachyotos. The ſhort-eared Owl. Owl.] Brit. Zoology, folio, plate B. 3. octavo, I. p. 156. Faun. Am. Sept. 9.

Severn River, N° 17 and 64.

Mouſe Hawk at Hudſon's Bay. It anſwers the deſcription and figure in the British Zoology; but its ears or long feathers do not appear. The ſmallneſs of the head has, probably, given occaſion to call it a hawk, though it does not fly about in queſt of prey, like other hawks (as the account from Severn River ſays); it ſits quiet on the ſtumps of trees, watching mice with all the attention of a domeſtic cat, being an inveterate enemy of thoſe little animals. It migrates ſouthward in autumn, and breeds along the coaſt. Its irides are yellow; its weight is 14 ounces; its length 16 inches, the breadth 2 feet.

STRIX.

STRIX, 5. Nyctea. 132. 6. Snowy Owl. Faun. Am. Sept. 9.

Churchill River, N° 7. White Owl.

It ſeems to be in its winter dreſs, as it is intirely white. The feet are covered with long white hair-like feathers to the very nails, but there are none on the ſoles or under parts of the toes.

STRIX, 6. Funerea. 133. 11. Canada Owl. Faun. Am. Sept. 9.

Severn River, N° 13. Churchill River, N° 11.

Cabeticuch, or *Cabaducutch*, is the Indian name of this bird. Linneus's deſcription anſwers perfectly. The male, which in the claſs of birds of prey is generally ſmaller, is, however, in this ſpecies, larger than the female, according to the account from Severn River. Its colour is likewiſe much blacker, and the ſpots more diſtinct. The eyes are large and prominent; the irides of a bright yellow. The weight is 12 ounces; its length 17 inches, the breadth 2 feet. It has only two young at one hatching.

STRIX, 7. Paſſerina. 133. 12. Little Owl. Brit. Zool. Faun. Am. Sept. 9.

(The number belonging to this bird is loſt, but it is moſt probably that from Severn River, N° 15. called *Shipomoſpiſh* by the natives).

The crown of the head is ſpeckled with white, as in the *Strix funerea*.

STRIX, 8. Nebulosa. *New species.* The grey Owl. Severn River, N° 36.

This fine non-descript owl lives upon hares, ptarmigans, mice, &c. It has two young at a time. The specimen sent over is said to be one of the largest. It is not described by any author. Its weight is 3 pounds, length 16 inches, breadth 4 feet.

3. LANIUS, Shrike. } 9. Excubitor. 135. 11. Great Butcher-bird. Brit. Zool. Cinereous Shrike. Faun. Am. Sept.

Severn River, N° 11.

White Whiskijohn at Hudson's Bay. The specimen is a male; it weighs two ounces and a half, is seldom found on the coast, but frequent about a hundred miles inland; and feeds on small birds. It corresponds with ours in every respect.

II. { Picæ. Pies. Faun. Am. Sept.

4. CORVUS, Crow. } 10. Canadensis. 158. 16. Cinereous Crow. Faun. Am. Sept. 9.

Severn River, N° 9 and 10.

These birds are called *Whiskijohn* and *Whiskijack* at the Hudson's Bay. They weigh 2 ounces; and are 9 inches long, and 11 broad. Their eyes are black, and their feet of the same colour. Their characters correspond with the Linnean description. They breed early in spring; their nests are made of sticks and grass,

graſs, and built in pine trees; they have two, rarely three, young ones at a time; their eggs are blue; they fly in pairs; the male and female are perfectly alike; they feed on black moſs, worms, and even fleſh. When near habitations or tents, they are apt to pilfer every thing they can come at, even ſalt meat; they are bold, and come into the tents to eat victuals out of the diſhes. They watch perſons baiting the traps for martins, and devour the bait as ſoon as they turn their backs. Theſe birds lay up ſtores for the winter, and are ſeldom ſeen in January, unleſs near habitations; they are a kind of mock-bird; when caught, they pine away and die, though their appetite never fails them.

Corvus, 11. Pica. 157. 13. Magpie. Brit. Zool. Faun. Am. Sept. 9.

Albany Fort, N° 5.

It is called *Oue-ta-kee aſke*, i. e. *Heart-bird*, by the Indians. It is a bird of paſſage, and rarely ſeen; it agrees, in all reſpects, with the European magpie, upon compariſon.

5. Picus, Woodpecker. } 12. Auratus. 174. 9. Gold-wing Woodpecker. Faun. Am. Sept. 10. Cateſby, I. 18.

Albany Fort, N° 4. the large Woodpecker.

The natives of America call this bird *Ou-thee-quan-nor-now*, from the yellow colour of the ſhafts of the quill and underſide of the tail feathers. It is a bird of paſſage; viſits the

neighbourhood of Albany Fort in April, leaves it in September; lays from four to six eggs in hollow trees, feeds on small worms and other insects. Its descriptions answer exactly.

PICUS, 13. Villosus, 175. 16. Hairy Woodpecker.
Faun. Am. Sept. 10. Catesby I. 19.
Severn River, N° 56.

The specimen sent over is a female, by its wanting the red on the head. The descriptions of Linneus and Brisson agree; only the two middlemost feathers are black, the next are of the same colour, but have a white rhomboidal spot near the tip; the next are black, with the upper half obliquely white, the very tip being black; the next after that are white, with a round black spot on the inner side close to the base, and the lower part of the shaft is black, the outermost feathers are quite white, the shaft only at the base being black.

14. Tridactylus. 177. 21. Three-toid Woodpecker.
Faun. Am. Sept.
Severn River, N° 8.

A female, weight 2 ounces, length 8 inches, breadth 13; eyes dark blue, legs black. It builds its nest in trees, lives in woods upon worms picked out of trees, is not very common at Severn River. The descriptions answer.

III. Gallinæ.

III. { Gallinæ.
Gallinaceous. Faun. Am. Sept.

6. Tetrao. { 15 Canadensis, 274. 3. } Faun. Am. Sept. 10.
Grous. { Canace, 275. 7. } Spotted Grous.
Gelinotte du Canada, male et femelle, Pl. enl. 131 et 132. Buffon Oiseaux II. p. 279. 4to. Brisson I. p. 203. t. 20. f. 1, 2, and p. 201. app. 10. Edwards, t. 118 and 71.
Severn River, N° 5. Woodpartridge.

These birds are all the year long at Hudson's Bay, and never change the colour of their plumage. The accounts from Hudson's Bay say, there is no material difference between the male and female; which must be a mistake, as they are really very different. Linneus's descriptions of the Tetrao Canadensis, and Canace, both answer to the specimens sent over, so that, after comparing them, I find they are only one and the same species. I suppose the dividing them into two, was occasioned by Brisson's and Edwards's descriptions, being taken from specimens sent from different parts of the continent of America, and perhaps caught at different seasons. Mr. de Buffon has, I find, the same opinion with me, and by comparing the drawings of Edwards, with those of the Planches enluminées, it is put beyond a doubt. These birds are very stupid, may be knocked down with a stick, and are frequently caught by the na-

I tives

tives with a ſtick and a loop. In ſummer they are good eating; but in winter they taſte ſtrongly of the pine ſpruce, upon which they feed during that ſeaſon, eating berries in ſummer. They live in pine woods, their neſts are on the ground; they generally lay but five eggs.

Tetrao, 16. Lagopus, 274. 4. White Grous. Faun. Am. Sept. 10. Ptarmigan. Br. Zool. Lagopéde de la Baye de Hudſon. Buffon Oiſeaux II. p. 276. Edw. t. 72.

Severn River. N° 1—4. Willow-partridges.

The Hudſon's Bay ptarmigan has been ſeparated from the European in the Britiſh Zoology, and afterwards by M. de Buffon: however, I muſt own, I cannot yet find the differences which they aſſign to theſe ſpecies. They contend that the Hudſon's Bay bird figured by Edwards is twice as big as the European ptarmigan; Mr. Edwards, I think, does not intimate this, when he ſays, the bird is of a middle ſize, between partridge and pheaſant; he on the contrary ſuppoſes them to be the ſame ſpecies. The Britiſh Zoology, after Willoughby, ſays, the ptarmigan's length is 13½ inches. The account from Severn River ſays it is 16¼ inches. The breadth in the Britiſh Zoology is ſaid to be 23 inches. The breadth in the Hudſon's Bay birds, according to the accounts from Severn River, is 23 inches. Willoughby's ptarmigan weighed 14 ounces; that in the Britiſh Zool.

Zool. illuſtr. t. 13. 19 ounces; that from the Hudſon's Bay (1½ ℔) 24 ounces. Theſe differences are of little conſequence, and far from increaſing the Hudſon's Bay bird to double the ſize of the European. The Britiſh Zoology ſays, there is a difference in the ſummer colours; but Mr. Edwards informs us, that he compared the Hudſon's Bay bird with the deſcriptions of former ornithologiſts, and found them to anſwer; he likewiſe aſſures us he had the ſame bird from Norway. Therefore I cannot help diſſenting from the Britiſh Zoology, in this one particular, and thinking with Linneus and Briſſon, that the European and Hudſon's Bay ptarmigans are the ſame, eſpecially as the colours vary very much in the different ſexes and at different ſeaſons. To this we may add the teſtimony of a gentleman well verſed in natural hiſtory, who, having had opportunities of comparing numbers of Hudſon's Bay and European ptarmigans, aſſured me that he did not ſee any difference between them. They go together in great flocks in the beginning of October, living among the willows, of which they eat the tops (whence they have got the name of willow partridges): about that time they loſe their beautiful ſummer plumage, and exchange it it for a ſnowy white dreſs, moſt providently adapted by its thickneſs to ſcreen them againſt the ſeverity of the ſeaſon, and by its colour againſt their enemies

the

the hawks and owls, against whose attacks they would otherwise find no shelter. Each feather is double, that is, a short one under a long one, to keep them warm. In the latter end of March, they begin again to change their plumage, and have got their full summer dress by the end of June. They breed every where along the coast, and have from nine to eleven young at a time; making their nests on the ground, generally on dry ridges. They are excellent eating, and so plentiful that ten thousand have been taken at Severn, York, and Churchill Forts. The method of netting or catching them, is as follows: a net made of jack-twine, twenty feet square, is laced to four long poles, and supported in front with the sticks, in a perpendicular situation; a long line is fastened to these supports, one end of it reaching to a place where a person lies concealed; several men drive the ptarmigans (which are as tame as chickens, especially on a mild, snowy day), towards the net, which they run to, as soon as they see it. The person concealed draws the line, by which means the net falls down, and catches 50 or 70 ptarmigans at once. They are sometimes rather wild, but grow better humoured (as Mr. Graham says) by being driven about, for they seldom forsake those willows which they have once frequented.

TETRAO. 17. Togatus, 275. 8. Shoulder-knot Grous. Grosse Gelinotte du Canada. Pl. enl. 104. Briss. I. 207. t. 21. f. 1. Buffon Oiseaux II. p. 287.

Severn River, N° 60 and 61. Albany Fort 1 and 2.

This bird answers the descriptions given of it by the ornithologists in all respects, and perfectly resembles the figure in Brisson, and in the Planches enluminées. It differs from Edwards's ruffed heathcock, t. 248. or Linneus's Tetrao umbellus, as the latter has not the shining black axillar feathers, or shoulder-knot, but a ferruginous one, is much less, and has brighter colours. M. de Buffon, however, thinks they are the same, and suspects at the same time, that the bird which he calls la grosse Gelinotte du Canada (and which is the same with the Society's specimens) is the female of Mr. Edwards's bird, t. 248. This conjecture is destroyed by the specimens now sent from Hudson's Bay, which by the accounts from thence are expressly said to be males. The shoulder-knot grouses bear the Indian name of *Puskee*, or *Puspuskee*, at Hudson's Bay, on account of the leanness and dryness of their flesh, which is extremely white, and of a very close texture, but when well prepared is excellent eating. They are pretty common at Moose Fort and Henly House, but are seldom seen at Albany Fort, or to the northward of the above places. In winter they feed upon ju-

 niper

niper tops, in summer on goose-berries, rasp-berries, currants, cranberries, &c. They are not migratory, staying all the year at Moose Fort; they build their nests on dry ground, hatch nine young at a time, to which the mother clucks, as our common hen does; and on the least appearance of danger, or in order to enjoy a comfortable degree of warmth, the young ones retire under the wings of their parent.

N. B. A specimen, which is supposed to be either a young bird or a female, wants the blueish black shoulder-knot; but it is the same in all other respects.

TETRAO, 18. Phasianellus. Linn. Syst. Nat. Ed. X. p. 160. n. 5. Edw. 117. Longtailed Grous. Faun. Am. Septentr. 10.

Severn River, N° 6 and 7. Albany Fort, N° 3.

This bird, which Mr. Edwards has drawn plate 117, was by Linneus in the tenth edition of his System, ranged as a new species of grous or tetrao, by the specific name of Phasianellus (alluding to the name of Pheasant which it bears at Hudson's Bay, and likewise to its pointed tail). He afterwards in the new or twelfth edition of the System, p. 273. makes it a variety of the great Cock of the Wood, or Tetrao Urogallus, probably from the account in Mr. Edwards, that the male struts very upright, is in general of a darker colour than the female, and has a glossy neck. These circumstances, however, are not sufficient to bring

bring them under the ſame ſpecies, *for* it is known that the males of all the grous tribe, and indeed of moſt of the gallinaceous birds, are uſed to ſtrut in a very ſtately manner, and that the colours of their plumage are much more diſtinct than thoſe of the females. But the ſpecific difference alone, which Linneus aſſigns to the cock of the wood, abſolutely excludes our Hudſon's Bay ſpecies; he calls it Tetrao pedibus hirſutis, cauda rotundata, axillis albis. Whoever examines Mr. Edwards's figure, and the ſpecimens now in the Society's poſſeſſion, will find the tail very ſhort, but pointed, the two middle feathers being half an inch longer than the reſt, (Mr. Edwards ſays two inches) and the axillæ, or ſhoulders, by no means white: beſides this difference, the colour and ſize of the Hudſon's Bay bird are likewiſe vaſtly different from thoſe of the cock of the wood. Its length is 17 inches, its breadth 24, and, as Mr. Edwards juſtly ſays, it is ſomewhat bigger than the common pheaſant. The great cock of the wood is as big as a turky; and its female, which is much leſs, however far exceeds our bird, it being 26 inches long, and 40 broad. See Britiſh Zool. octavo, p. 200. The figures given of the female of the T. Urogallus, or great cock of the wood, in the Br. Zool. folio, plate M *, and the Planche enluminée 75, will ſerve upon compariſon as a convincing proof of the vaſt difference there is between the Hudſon's Bay pheaſant grous and the European cock

 of

of the wood. The figure, which Mr. Edwards has given of the former bird, does not exactly correspond with the Society's specimen, as he has represented the marks on the breast half-moon shaped, though they are heart-shaped as those on the belly in the dried bird; that is, they are white spots, with a pale brownish yellow cordated brim. Nor can I agree with Mr. Edwards, when he calls this bird the long-tailed grous from Hudson's Bay; for its tail is really very short, in comparison with that of other grouse, and its smallness and acuteness afford one of the most distinguishing characters of the species.

The native Indians call these pheasant grouses, *Oc-kiss-cow*: they are found all the year long, amongst the small juniper bushes, of which the buds are their principal food, as also the buds of birch in winter, and all sorts of berries in summer. They never vary their colours; nor is there any great difference between the male and female, except in the caruncula or comb over the eye, which in the male is an inch long, and $\frac{1}{4}$ of an inch high. The account from Albany Fort adds, that the colour of the male is somewhat browner, and almost a chocolate on the breast. Their flesh is of a light brown, exceeding juicy, and they are very plump. They lay from 9 to 13 eggs; their young can run almost as soon as they are hatched; they make a piping noise somewhat like a chicken. The cock has a shrill crowing note, not very loud;

 but

but when diſturbed, or whilſt flying, he makes a repeated noiſe of cuck, cock. They are moſt common in winter at Albany Fort.

Before I leave the genus of grouſes, I muſt obſerve that their feet have a peculiarity, taken notice of by few authors; the toes, in ſeveral ſpecies, have on each ſide a row of ſhort flexible teeth, like thoſe of a comb; ſo that the toes appear pectinated. The ſpecies, which are known to have ſuch pectinated toes, are,

1. The great Cock of the Wood, *Tetrao Urogallus*, Linn.
2. The Black Cock, *T. Tetrix*, Linn.
3. The Spotted Grous, and { *T. Canadenſis*, *T. Canace*, Linn.
4. The Ruffed Grous, *T. Umbellus*, Linn.
5. The Shoulder-knot Grous, *T. Togatus*, Linn.
6. The Pheaſant Grous, *T. Phaſianellus*.
7. The Hazel Hen, *T. Bonaſia*, Linn.
8. The Pyrenæan Grous, *T. Alchata*, Linn.

This is a circumſtance, which ought to be attended to in all other ſpecies of grouſes, as it may in time afford a diſtinguiſhing character for a diviſion in this great genus; the ptarmigan, or *T. Lagopus*, Linn. is without theſe teeth.

IV. {Columbæ.
{Columbine. Faun. Am. Sept.

7. COLUMBA, } 19. Migratoria. 285. 36. Migratory Pigeon. } Pigeon. Catesb. I. 23. Kalm II. p. 82, t. Passenger Pigeon, Faun. Am. Sept. 11. Severn River, N° 63. Wood-pigeon.

These pigeons are very scarce so far northward as Severn river, but abound near Moose-fort, and further inland to the southward. Their common food are berries and juniper buds in winter; they fly about in great flocks, and are reckoned good eating. This account is confirmed by Kalm in his travels (English edition) Vol. II. p. 82 and 311. They hatch only two eggs at a time, and their nests are built in trees. Their eyes are small and black, the irides yellow, the feet red: the neck finely glossed with purple, brighter in the male. They weigh 9 ounces.

V. {Passeres.
{Passerine. Faun. Am. Sept.

8. Alauda. } 20. Alpestris. 289. 10. Klein, Hist. of Lark. } Birds, 4to. p. 73. Shore Lark, Faun. Am. Sept. 12. Catesb. I. 32. Albany Fort, N° 6.

This species is indifferently described by Linneus, who says that all the tail-feathers on their inner web are white, (*rectricibus dimidio interiore albis*); though it does not appear that he saw a specimen of it himself. Both the quill

quill and tail-feathers are duſky, and in both the outermoſt feather only has a white exterior margin. The coverts of the tail are of a pale ferruginous colour, and two of them are nearly as long as the tail itſelf. The ſcapulars are ferruginous; in the male, the head and whole back have a tinge of the ſame colour, marked with duſky ſtreaks; in the female, the back is grey, and the duſky ſtripes of a darker hue. The crown of the head is black in the male, duſky in the female; the forehead is yellow, the bill and feet are black, the belly of a dirty reddiſh white. Theſe larks are migratory, they viſit the environs of Albany Fort in the beginning of May, but go further northward to breed: they feed on graſs-ſeeds, and buds of the ſprig-birch; run into ſmall holes, and keep cloſe to the ground, from whence the natives give them the name of *Chi-chup-pi-ſue*.

9. Turdus. Thruſh. } 21. Migratorius, 292. 6. American Fieldfare. Kalm II. p. 90. Faun. Am. Sept. II. Cateſby I. 29.

Severn River, N° 59. Albany Fort, 7, 8, 9.

The deſcriptions of theſe birds in various authors coincide with the ſpecimens; at Severn River they appear at the beginning of May, and leave the environs before the froſt ſets in. At Mooſe Fort, in the north latitude 51°. they build their neſt, lay their eggs, and hatch their young in the ſpace of fourteen days; but at York fort and Severn ſettlement this is

done

done in 25 hours: they build their nests in trees, lay four beautiful light-blue eggs, feed on worms and carrion: when at liberty they sing very prettily, but confined in a cage, they lose their melody. There is no material distinction between the male and female. Their weight is 2½ ounces, the length 9 inches, and the breadth 1 foot; they are called red birds at Hudson's Bay; their Indian name is *Pee-pee-chue*.

Turdus, 22.

Severn River, N° 54 and 55, male and female.

From the striking similarity with our blackbird, the English at Hudson's Bay have given this bird the same name. However, upon a close examination, I find the difference very great between our European blackbird, and the Hudson's Bay or American one. The plumage of the male, instead of being deep black without any gloss, as in ours, has a shining purple cast, not unlike the plumage of the *Gracula Quiscula*, Linn. or shining Gracule, Faun. Am. Sept.; or the Maize thief, of Kalm. The female indeed is very like our female blackbird, being of a dusky colour on the back, and a dark grey on the breast. The feet and bill are quite black in both sexes; the former have the back claw almost as long again as any of the other claws. There are no vestiges of yellow palpebræ in either the male or the female; the bill in both is strong, smooth, and subulated; the upper

upper mandible being carinated, but very little arched, and without any tooth or indenture whatever, on the lower side. The nostrils are as in other thrushes. This bird has no bristles at the base of its bill, its feet have such segments as Scopoli in the Annus I. Historico-Naturalis attributes to the stares. Instead of being solitary and living retired like the European blackbirds, these American ones come in flocks to Severn River in June, live among the willows, build in all kinds of trees, and return to the southward in autumn. They feed on worms and maggots; their weight is 2¼ ounces, and they are nine inches long, and one foot broad. One that was kept twelve months in a cage pined away, and died. Notwithstanding these circumstances, I cannot help remaining undetermined with regard to this bird, which at first sight is like the blackbird, has the bill of a thrush, and the feet and gregarious nature of a stare. It is to be hoped, that future accounts from Hudson's Bay may inform us further, of the nature of this bird, its time of incubation, the number of eggs it lays, and the colour of those eggs, together with the note of the bird, the difference and characteristick marks of both the male, and female, and other circumstances, which may serve to determine to what genus and species we are to refer this bird.

10. LOXIA, Grosbeak. { 23. Curvirostra, 299. 1. Crossbill. Br. Zool. Faun. Am. Sept. 11. The small variety.

Severn River, N° 27 and 28.

This bird comes to Severn River the latter end of May, breeds more to the northward, and returns in autumn, in its way to the south, departing at the setting in of the frost. The irides in the male are of a beautiful red, in the female yellow: the weight is said to be 10 ounces (probably by mistake for 1 ounce, as it is impossible so small a bird should weigh more), the length is 6 inches, the breadth 10.

24. Enucleator, 299. 3. Pine Grosbeak. Br. Zool. and Faun. Am. Sept. Edw. 123, 124. Pl. enl. 135. f. 1.

Severn River, N° 29, 30.

It answers to the descriptions and figures of the ornithologists pretty well; only Edwards's female has the red too bright, which is rather orange in our specimen, on the head, neck, and coverts of the tail. This bird only visits the Hudson's Bay settlements in May, on its way to the north, and is not observed to return in autumn; its food consists of birch-willow buds, and others of the same nature; it weighs 2 ounces, is 9 inches long, and 13 broad.

11. EMBERIZA. Bunting. {25. Nivalis. 308. 1. Greater Brambling, Br. Zool. Snowbird Snowflake, ibid. Snow-bunting. Faun. Am. Sept. 11.

Severn River, N° 24—26.

The bird, in summer dress, corresponds exactly with the description of the greater brambling, Br. Zool. The description of the snowflake, or the same bird in winter dress, ibid. vol. IV p. 19. is somewhat different, perhaps owing to the different seasons the birds were caught in, as it is well known they change their colour gradually. They are the first of the migratory birds, which come in spring to Severn settlement; in the year 1771 they appeared April the 11th, stayed about a month or five weeks, and then proceeded further northward in order to breed there; they return in September, stay till the cold grows severe in November, then retire southward to a warmer climate. They live in flocks, feed on grass-seeds, and about the dunghills, are easily caught under a small net, some oatmeal being strewed under it to allure them; they are very fat, and fine eating. The weight is 1 ounce and 5 drams, the length 6¾ inches, and the breadth 10 inches.

EMBRIZA. 26. Leucophrys. *New Species.* White Crowned Bunting.

Severn River, N° 50. Albany Fort, 10.

This elegant little species of Bunting is called a hedge sparrow at Hudson's Bay, and has

not hitherto been deſcribed. It viſits Severn ſettlement in June, and feeds on graſs-ſeeds, little worms, grubs, &c. It weighs ¾ of an ounce, and is 7¼ inches long, and 9 inches broad; the bill and legs are fleſh-coloured; the male is not materially different from the female, its neſts are built in the bottom of willow buſhes, it lays three eggs of a chocolate colour. It viſits Albany Fort in May, breeds there, and leaves it in September.

12. FRINGILLA, Finch. { 27. Lapponica. 317. 1. Faun. Succ. 235.

Severn river, N° 52.

It is called *Tecurmaſhiſh*, by the natives at Hudſon's Bay. The deſcription in Linneus's Fauna Succica coincides exactly with the ſpecimen; that in his Syſtem anſwers very nearly: Mr. Briſſon's deſcription (though he quotes Linneus, and Linneus quotes him) is widely different. The ſpecimen ſent over is a female; the males have more of the ferruginous colour on the head; the eyes are blue, the legs dark brown. It is only a winter inhabitant near Severn river, appears not before November, and is commonly found among the juniper trees; it weighs ¾ of an ounce, its length is 5 inches, and its breadth 7.

FRINGILLA. 28. Linaria. 322. 29. Leſſer red headed Linnet. Br. Zool.

Severn River, N° 23.

The deſcriptions of Linneus, Briſſon, and the Britiſh Zoology, anſwer perfectly well. The figure in Planche enluminée 151. f. 2. has a quite ferruginous back contrary to all the deſcriptions and the ſpecimen before us, in which all the feathers on the back are duſky, edged with dirty white.

29. Montana, 324. 37. Mountain Sparrow, Tree Sparrow. Br. Zool. Edw. 269. Briſſon III. p. 79. Faun. Am. Sept.

Severn River, N° 20.

This ſeems to be a variety, as its tail is rather longer than uſual, and forked; it anſwers nearly to the deſcriptions given by the ornithologiſts, and ſeems to be a female, as it has no black under the throat and eyes, and no white collar. The bill and legs are black, the eyes blue. At Severn ſettlement it arrives in May, goes to breed further northwards, and returns in autumn: the weight is ¾ of an ounce, the length 6¼ inches, and breadth 10. I was inclined to make this bird a new ſpecies, on account of the many differences between it and the mountain ſparrow; but conſidering the ſpecimen ſent over was not in the beſt order, and might be a female, I thought it beſt to leave it where it is, till we are better informed.

FRIN-

FRINGILLA. 30. Hudsonias. *New Specimen.* Severn River, N° 18.

This is certainly a nondescript species; it only visits Severn settlement in summer, not being seen there before June, when it stays about a fortnight, goes further to the northward to breed, and passes by Severn again in autumn on its return south. It is very difficult to procure, and therefore it could not be determined whether the specimen was a male or female. It frequents the plains, and lives on grass-seeds; it weighs ½ an ounce, is 6¼ inches long, and 9 inches broad: it has a small blue eye, and a whitish bill faintly tinged with red; the whole body is blackish, or of a soot colour, the belly alone with the two outermost tail feathers on each side being white. It is to be wished that more specimens and circumstantial accounts of this bird were sent over, which would enable us to determine its character with more precision.

13. MUSCICAPA, Flycatcher. { 31. Striata. *New Species*, Striped Flycatcher.
Severn River, N° 48 and 49. Male and Female.

This species visits Severn river only in summer, feeding on grass-seeds, etc.; it weighs half an ounce, is 5 inches long, and seven broad; the male is widely different from the female: this species is entirely nondescript.

14. MOTACILLA, Wagtail. { 32. Calendula. 337. 47. Ruby crowned Wren. Edw. 254.
Faun. Am. Sept.

(The number belonging to this bird is lost; however, it is most probably that sent from Severn river, N° 53.)

It answers to the descriptions and the figure of Edwards; its weight is 4 drams, its length 4 inches, and its breath 5. It migrates, feeds on grass-seeds and the like, and breeds in the plains; the number of eggs is not known.

15. PARUS, Titmouse. { 33. Atricapillus. 341. 6. Black Cap Titmouse.
Albany Fort, N° 11.

The description given by Linneus answers, and so does M. Brisson's in most particulars, except that the quill-feathers are not white on the inside. These birds stay at Albany Fort all the year, yet seem most numerous in the coldest weather; probably being then more in want of food, they come nearer the settlements, in order to pick up all remnants. They feed on flies and small maggots, and likewise on the buds of the sprig-birch, in which they perhaps only search for insects; they make a twittering noise, from which the native call them *Kiss-kiss-ke-shish*.

PARUS.

PARUS. 34. Hudsonicus. *New Species.* Hudson's Bay Titmouse.

Severn River, N° 12.

This new species of titmouse, is called *Peche-ke-ke-shish*, by the natives. They are common about the juniper bushes, of which the buds are their food; in winter they fly about from tree to tree in small flocks, the severest weather not excepted. They breed about the settlements, and lay 5 eggs; they have small eyes, with a white streak under them, and black legs: the male and female are quite alike; they weigh half an ounce, are 5¼ inches long, and 7 inches broad.

16. HIRUNDO, Swallow. } 35.

Severn River, N° 58.

The swallows build under the windows, and on the face of steep banks of the river, they disappear in autumn; and the Indians say, they were never found torpid under water, probably because they have no large nets to fish with under the ice. The specimen sent answers in some particulars to the description of the Martin, Hirundo Urbica, Linn. but seems to be smaller, and has no white on the rump. I have, therefore, thought it best to leave the species undetermined, till further informations are received from Hudson's Bay, on this subject.

2. WATER-

2. Water-Birds.

VI. { Grallæ,
Clovenfooted. Faun. Am. Sept.

17. Ardea, { 36. Canadensis. 234. 3. Edw. 133.
Heron. { Canada Crane. Faun. Am. Sept. 14.
Severn River, N° 35. Blue Crane.

The account from Severn settlement says, there is no material difference between the male and female; however, the specimen sent over, I take to be a female, as its plumage is in general duller than that figured by Edwards, and as the last row of white coverts of the wing are wanting. These cranes arrive near Severn in May, have only two young at a time, retire southward in autumn; frequent lakes and ponds, and feed on fish, worms, &c. They weigh seven pounds and a half, are 3¼ feet long, and 3 feet 5 inches broad; the bill is 4 inches long, the legs 7 inches, but the leg and thigh 19.

Ardea. 37. Americana, 234. 5. Hooping Crane. Edw. 132. Catesby, l. 75. Faun. Am. Sept. 14.
York Fort.

Edwards's figure is very exact; Catesby's is not so good, as it represents the bill too thick towards the point.

38. Stellaris, 239. 21. *Varietas.* The Bittern, Br. Zool. Edw. 136. Faun. Am. Sept. pag. 14*. Severn River, N° 64.

At first sight, I thought the specimen sent from Hudson's Bay, was a young bird; but upon nearer examination and comparing it with Mr. Edwards's account and figure, I take it to be a variety of the common bittern peculiar to North America; it is smaller, but upon the whole very much resembles our bittern. Mr. Edwards's measurements and drawings correspond very well with the specimen.

This bird appears at Severn river the latter end of May, lives chiefly among the swamps and willows, where it builds its nest, and lays only two eggs at a time; it is very indolent, and, when roused, removes only to a short distance.

18. SCOLOPAX, Woodcock. { 39. Totanus. 245. 12. Spotted Woodcock. Faun. Am. Sept. 14. Albany Fort, N° 16.

This bird is called a yellow leg at Albany fort, from the bright yellow colour of the legs, especially in old birds; a circumstance, in which it varies from the descriptions of Linneus and Brisson, probably because they described

* In the Faunula America Septentrionalis, p. 14. the synonym of Ardea Hudsonias, Linn. has by mistake been annexed to the bittern, and likewise pl. 135 of Edwards has been quoted instead of plate 136. They are two very different birds.

ſcribed from dried ſpecimens, in which the yellow colour always changes into brown. It agrees in other reſpects perfectly well with the deſcriptions: it comes to Albany fort in April or beginning of May, and leaves it the latter end of September. It feeds on ſmall ſhell fiſh, worms, and maggots; and frequents the banks of rivers, ſwamps, &c. It is called by the natives *Sa-ſa-ſhew*, from the noiſe it makes.

SCOLOPAX. 40. Lapponica. 246. 15. Red Godwit. Br. Zool. Faun. Am. Sept. 14. Ed. 138. Churchill River, N° 13.

Linneus deſcribes this bird very exactly in his Syſtema Naturæ: the middle of the belly has no white in the Society's ſpecimen, as that had from which the deſcription in the Br. Zool. octavo I. p. 353, 354, was taken. All the other characters correſpond.

SCOLOPAX. 41. Borealis. *New Species.* Eſkimaux Curlew. Faun. Am. Sept, 14.
Albany Fort, N° 15.

This ſpecies of Curlew, is not yet known to the ornithologiſts; the firſt mention is made of it in the Faunula Americæ Septentrionalis, or catalogue of North American animals. It is called *Wee-kee-me-naſe-ſu*, by the natives; feeds on ſwamps, worms, grubs, &c; viſits Albany Fort in April or beginning of May; breeds to the northward of it, returns in Au-

 guſt,

guſt, and goes away ſouthward again the latter end of September.

19. TRINGA, Sand-piper. { 42. Interpres. 248. 4. Turnſtone. Edw. 141. Faun. Am. Sept. 14.

Severn River, N° 31 and 32.

This ſpecies is well deſcribed by the ornithologiſts; its weight is 3½ ounces, the length 8½ inches, and the breadth 17 inches; it has four young at a time; its eyes are black, and the feet of a bright orange: this bird frequents the ſides of the river.

43. Helvetica. 250. 12, Briſſon. Av. V. p. 106. t. 10. f. 2.

(The number was loſt, perhaps it is N° 17, from Fort Albany; upon that ſuppoſition the account is as follows: "the natives call it "*Waw-puſk-abrea-ſhiſh*, or white bear bird; "it feeds on berries, inſects, grubs, worms, "and ſmall ſhell-fiſh; viſits and leaves Al-"bany fort at the ſame time with the *Sco-"lopax Totanus*, and *Borealis*.")

I find this bird anſwers very well to its deſcription; the throat, breaſt, and upper part of the belly are blackiſh, as in the deſcriptions, but mixed with white lunulated ſpots, which are neither deſcribed nor expreſſed in M. Briſſon's figure, and may be owing to the difference of ſex, or climate.

VII.

VII. { ANSERES.
Webbed-footed. Faun. Am. Sept.

29. ANAS, { 44. Marila. 196. 8. Scaup Duck. Br.
Duck, { Zool. Faun. Am. Sept. 17.

Severn River, N° 44 and 45. Fishing Ducks.

Linneus's description, and the figure in the Br. Zoology, folio, plate Q. p. 153, agree perfectly well with the specimens. The female, as Linneus observes, is quite brown, the breast and upper part of the back being of a glossy reddish brown; the speculum of the wing and the belly are white. The eyes of the male have very bright yellow irides; those of the female are of a faint dirty yellow. The female is two ounces heavier than the male, which weighs one pound and an half, is 16½ inches long, and 20 inches broad.

ANAS. 45. Nivalis. Snow Goose. Faun. Am. Sept. p. 16. Lawson's Carolina. Anser niveus Briss. VI. 288. Klein. Anser nivis. Schwenkfeld, Marsigli. Danub. p. 802. t. 49.

Severn River, N° 40, and a young one, N° 41. white Goose.

These white geese are very numerous at Hudson's Bay, many thousands being annually killed with the gun, for the use of the settlements. They are usually shot whilst on the wing, the Indians being very expert at that exercise, which they learn from their youth; they weigh five or six pounds, are

2¾ feet.

2½ feet long, and 3½ broad; their eyes are black, the irides ſmall and red, the legs likewiſe red; they feed along the ſea, and are fine eating; their young are bluiſh grey, and do not attain a perfect whiteneſs till they are a year old. They viſit Severn river firſt in the middle of May, on their journey northward, where they breed; return in the beginning of September, with their young, ſtaying at Severn ſettlement about a fortnight each time. The Indian name is *Way-way*, at Churchill river. Linneus has not taken notice of this ſpecies.

ANAS. 46. Canadenſis. 198. 14. Canada Gooſe. Faun. Am. Sept. 16. Edw. 151. Cateſby I. 92, &c.

Severn River, N° 42.

The Canada geeſe are very plentiful at Hudſon's Bay, great quantities of them are ſalted, but they have a fiſhy taſte. The ſpecimen ſent over agrees perfectly with the deſcriptions and drawings. At Hudſon's Bay this ſpecies is called the *Small Grey Gooſe*. Beſides this, and the preceding white gooſe, Mr. Graham, the gentleman who ſent the account from Severn ſettlement, mentions three other ſpecies of wild geeſe to be met with at Hudſon's Bay; he calls them,

1. The large Grey Gooſe.
2. The Blue Gooſe.
3. The Laughing Gooſe.

The

The first of these, the large grey goose, he says, is so common in England, that he thought it unnecessary to send specimens of it over. It is however presumed, that though Mr. Graham has shewn himself a careful observer, and an indefatigable collector; yet, not being a naturalist, he could not enter into any minute examination about the species to which each goose belongs, nor from mere recollection know, that his grey goose was actually to be met with in England. A natural historian, by examination, often finds material differences, which would escape a person unacquainted with natural history. The wish, therefore, of seeing the specimens of these species of geese, must occur to every lover of that science. Mr. Graham says, the large grey geese are the only species that breed about Severn river. They frequent the plains and swamps along the coast. Their weight is nine pounds.

The blue goose is as big as the white goose; and the laughing goose is of the size of the Canada or small grey goose. These two last species are very common along Hudson's Bay to the southward, but very rare to the northward of Severn river. The Indians have a peculiar method of killing all these species of geese, and likewise swans. As these birds fly regularly along the marshes, the Indians range themselves in a line across the marsh, from the wood to high water mark, about musket shot from eath other,

so

ſo as to be ſure of intercepting any geeſe which fly that way. Each perſon conceals himſelf, by putting round him ſome bruſh wood; they likewiſe make artificial geeſe of ſticks and mud, placing them at a ſhort diſtance from themſelves, in order to decoy the real geeſe within ſhot: thus prepared, they ſit down, and keep a good look out; and as ſoon as the flock approaches, they all lie down, imitating the call or note of geeſe, which theſe birds no ſooner hear, and perceive the decoys, than they go ſtraight down towards them; then the Indians riſe on their knees, and diſcharge one, two or three guns each, killing two or even three geeſe at each ſhot, for they are very expert. Mr. Graham ſays, he has ſeen a row of Indians, by calling round a flock of geeſe, keep them hovering among them, till every one of the geeſe was killed. Every ſpecies of geeſe has its peculiar note or call, which muſt greatly increaſe the difficulty of enticing them.

ANAS. 47. Albeola. 199. 18. The Red Duck. Faun. Am. Sept. 17. Edw. t. 100. Sarcelle de la Louiſiane. Briſſon VI. t. 41. f. 1.

Severn River, N° 37 and 38. Fiſhing Birds.

The deſcriptions and figures anſwer very well with the male, except that the three exterior feathers are not white on the outſide, but all duſky.

The female is not deſcribed by any one of the ornithologiſts; and therefore deſerves to be noticed,

noticed, to prevent future miſtakes. The whole bird is duſky, a few feathers on the forehead are ruſty, and ſome about the ears of a dirty white; the breaſt is grey, the belly and ſpeculum in the wings white; the bill and legs are black. They viſit Severn ſettlement in June, build their neſts in trees, and breed among the woods, and near ponds; the weight of the female is one pound, its length 14 inches, and its breath 21.

ANAS. 48. Clangula. 201. 23. Golden Eye.
Br. Zool. Faun. Am. Sept. 16.
Severn River, N° 51.

Theſe birds frequent lakes and ponds, and breed there: they eat fiſh and ſlime, and cannot riſe off the dry land. The legs and irides are yellow; their weight is 2¼ pounds, and their meaſure 19 inches in length, and two feet in breadth. The ſpecimen ſent is the male.

ANAS. 49. Perſpicillata. 201. 25. Black Duck.
Faun. Am. Sept. 16. Edw. 155.
Churchill River, N° 14.

This ſpecies is exactly deſcribed, and well drawn by Edwards. The Indians call it *She-ke-ſu-partem*. It ought to come into the firſt diviſion of Linneus's ducks, "roſtro baſi "gibbo," as its bill is really very unequal at the baſe.

ANAS. 50. Glacialis. 203. 30. and Hyemalis, 202. 29. Edw. t. 156. Swallow-tail. Br. Zool. Faun. Am. Sept. 17.

Churchill River, N° 12.

At Churchill River the Indians call this ſpecies, *Har-bar-vey*; it correſponds with Edwards's deſcription and drawing, plate 156, but differs much from Linneus's inexact deſcription of the Anas Hyemalis, to which he, however, quotes Edwards. Upon the whole it is almoſt without a doubt that the bird repreſented by Edwards, plate 280, and Br. Zool. folio, plate Q. 7, and quoted by Linneus for his Anas glacialis, is the male, and that the bird figured by Edwards t. 156, and quoted by Linneus for the Anas Hyemalis, is the female, of one and the ſame ſpecies. Linneus mentions a white body (in his Anas hyemalis) which in Edw. Tab. 156, and in the Society's ſpecimen, is all brown and duſky, except the belly, temples, a ſpot on the back of the head, and the ſides of the rump, which are white. Linneus ſays, that the temples are black; in the ſpecimen now ſent over, and in Mr. Edwards's figure, which Linneus quotes, they are white; the breaſt, back, and wings, are not black as he ſays, but rather brown and duſky. A further proof, that Linneus's Anas Glacialis and Hyemalis are the ſame, is that the feet in both t. 156 and 280 of Edwards are [illegible] and the bill black, with an orang[illegible]

ANAS. 51. Crecca. 204. 33. *Varietas.* Teal. Br. Zool. Faun. Am. Sept. 17.

Severn River, N° 33, 34. Male and female.

This is a variety of the teal, for it wants the two white ſtreaks above and below the eyes; the lower one indeed is faintly expreſſed in the male, which has alſo a lunated bar of white over each ſhoulder; this is not to be found in the European teal. This ſpecies is not very plentiful near Severn river; they live in the woods and plains near little ponds of water, and have from five to ſeven young at a time.

ANAS. 52. Hiſtrionica. 204. 35. Harlequin Duck. Faun. Am. Sept. 16. Edw. t. 99.

This bird had no number fixed to it; it agrees perfectly with Edwards's figure.

ANAS. 53. Boſchas. 205. 40. Mallard Drake. Faun. Am. Sept. Br. Zool.

Severn River, N° 39.

It is called Stock Drake at Hudſon's Bay, and correſponds in every reſpect with the European one, upon compariſon.

21. PELECANUS. 54. Onocrotalus. [illegible] *Pelican.* [illegible]

York Fort.

This variety of the pelican, agrees in every pa- [illegible] Linnæus's oriental pelecan (Pelecanus

canus onocrotalus orientalis), but has a peculiar tuft or fringe of fibres in the middle of the upper mandible, something nearer the apex than the base. This tuft has not been mentioned by any author, and is likewise wanting in Edwards's pelican, t. 92. with which the Society's specimen corresponds in every other circumstance. The P. Onocrotalus occidentalis, Linn. or Edw. t. 93 American pelican, is very different from it: the chief differences are the colour, which in our Hudson's Bay bird is white, but in Edwards's is of a greyish brown; and the size, which in the white bird is almost double of the brown one. The quill-feathers are black, and the shafts of the larger ones white. The *Alula*, or bastard wing, is black. The bill and legs are yellow.

22. COLYMBUS. } 55. Glacialis. 221. 5. Northern
* Diver, } Diver. Br. Zool. Faun. Am. Sept. 16.

Churchill River, N° 8. called a Loon there.

This bird is well described and drawn in the British Zoology, in folio.

* * } 56. Auritus, α. 222. 8. Edw. 145.
Grebe. } Eared Grebe. Faun. Am. Sept. 15.

Severn River, N° 43.

This is exactly the bird drawn by Edwards, t. 145. The specimen sent over is a female. It differs much from our lesser crested Grebe.

Br.

Br. Zool. octavo I. p. 396, and Br. Zool. illustr. plate 77. fig. 2. and Ed. 96. fig. 2. However, in both these works, it is looked on only as a variety, or different in sex. Mr. Graham has the same opinion. It lives on fish, frequenting the lakes near the sea coast. It lays its eggs in water, and cannot rise off dry land. It is seen about the beginning of June, but migrates southward in autumn. It is called *Sekeep*, by the natives. Its eyes are small, the irides red; it weighs one pound, and measures one foot in length, and one third more in breadth.

23. LARUS. Gull. } 57. Parasiticus. 226. 10. Arctic Gull. Br. Zool. Faun. Am. Sept. 16. Edw. 148. 149.

Churchill River, N° 15.

This species is called a *Man of War*, at Hudson's Bay. It seems to be a female, by the dirty white colour of its plumage below; it agrees very well with Edwards's drawing, and that in the Br. Zool. illustr.

24. STERNA. Tern. } 58. Hirundo *(Variety)*, 227. 2. The greater Tern. Br. Zool. Faun. Am. Sept.

(The number belonging to this bird is lost, perhaps it is N° 17, from Churchill River, called

"A sort

"A sort of Gull, called Egg-breakers, by
"the natives.")

The feet are black; the tail is shorter and much less forked than that described and drawn in the Br. Zool. The outermost tail-feather likewise wants the black, which that in the British Zoology has. In other respects it is the same.

DESCRIPTIONES Avium Rariorum e Sinu Hudſonis.

1. Falco sacer.

Falco, cerâ pedibuſque coeruleis, corpore, remigibus rectricibuſque fuſcis, faſciis pallidis; capite, pectore & abdomine albis, maculis longitudinalibus fuſcis.

Habitat ad ſinum Hudſonis et in reliqua America Septentrionali; victitat Lagopodibus & Tetraonum ſpeciebus.

Descr. *Magnitudo* Corvi.

Roſtrum, cera, pedes coerulea; roſtrum breve, curvum, coeruleo-atrum; mandibula utraque, baſi pallide coerulea, apice nigreſcente, utraque emarginata.

Caput tectum pennis albidis, maculis longitudinalibus, fuſcis.

Oculi magni; irides flavæ.

Gula alba, fuſco-maculata.

Dorſum et tectrices alarum, plumis fuſcis, ferrugineo-pallide marginatis, maculatiſque, maculis rachin non attingentibus.

Pectus, venter, criſſum, tectrices alarum inferiores, & femora alba; maculis longitudinalibus nigro-fuſcis.

Remiges fuſco-nigræ, [illegible] duo; primores apicibus margine albis, maculis fer-

rugineo-pallidis, intra majoribus, tranſverſis, extra minoribus, rotundatis.

Rectrices duodecim, ſupra fuſcæ, faſciis circiter duodecim & apice albidis; infra cinereæ, faſciis albidis.

2. Strix Nebuloſa.

Strix capite lævi, corpore fuſco, albido undulatim ſtriato, remige ſexto longiore, apice nigricante.

Habitat circa Sinum Hudſonis, victitat Leporibus, Lagopodibus, Muribuſque.

Descr. *Roſtrum* fuſco-flavum, mandibula ſuperiore ſuperius magis flava.

Oculi magni, iridibus flavis.

Caput facie cinerea, e pennis fuſco et pallide cinereo alternatim ſtriatis. Pone hasce pennas collum verſus eſt ordo plumularum fuſcarum ad utramque genam, ſemicirculum nigrum efficiens.

Occiput, cervix, et collum fuſca, pennis, marginibus albo-maculatis.

Pectus albidum, maculis longitudinalibus tranſverſiſque fuſcis.

Abdomen album, ſuperius uti pectus maculis longitudinalibus, ſed inferius ſtriis tranſverſis notatum.

Dorſum totum et tectrices alæ, caudæque confertim ex fuſco & albido undulato-ſtriatæ.

Alæ fuſcæ; remiges primores fuſci, griſeo tranſverſim faſciati, faſciis latis nebuloſis. Remex ſextus, reliquis longior, apice

magis

magis nigricans; primus vero reliquis primoribus brevior. Remiges reliqui pallidiores, obscurius fasciati.

Cauda rotundata, rectricibus duodecim: duæ intermediæ paullo longiores, totæ cinerascente albido fuscoque undulatim striatæ, lineis duplicatis fuscis transversis pluribus. Rectrices reliquæ fuscæ albido substriatæ.

Pedes tecti pennis albidis fusco-striatis.

Magnitudo fere Strigis Nycteæ, Linn.

Longitudo unciarum 16 pedis Anglicani.

Latitudo pedum quatuor.

Pondus librarum trium.

3. TETRAO PHASIANELLUS.

Linn. Ed. X. p. 160. n. 5.

TETRAO pedibus hirsutis, cauda cuneiformi, remigibus nigris, exterius albo-maculatis.

Habitat ad Sinum Hudsonis.

DESCR. *Magnitudo* fere Tetraonis Tetricis. Linn.

Rostrum nigrum.

Oculorum irides avellaneæ.

Caput, collum & dorsum testacea, nigro transversim fasciata: macula albida inter rostrum et oculos: latera colli notata maculis rotundatis albidis.

Dorsum testaceum, plumis omnibus late nigro-fasciatis.

 Uropygium

Uropygium magis albido-cinereum, nigredine fimbriata fecundum rachin plumarum.

Pectus & Venter albida, maculis cordatis fufco-teftaceis in ventre faturatioribus.

Alarum tectrices dilute teftacco, nigro, alboque tranfverfim fafciatæ, maculis pluribus rotundis albis. *Remiges* primores nigri, latere exteriore albo-maculati; fecundarii fufci, apice & ad marginem exteriorem albo fubfafciati: poftremi vero teftacco fafciati, apice tantum albi.

Rectrices breves, exteriores pallide fufcæ, apice albæ, duæ intermediæ reliquis longiores, teftaceo-maculatæ.

Pedes plumis albo-grifeis vefti digitis pectinatis.

Longitudo unciarum 16 pedis Anglicani.

Latitudo pedum duorum.

4. Emberiza leucophrys *.

Emberiza remigibus rectricibufque fufcis, capite nigro, fafcia verticis, fuperciliifque niveis.

Habitat in America Boreali ad Sinum Hudfonis.

Descr. *Magnitudo* circiter *fringillæ cœlibis.*

Roftrum rubrum, f. carnei coloris: Nares fubrotundæ.

Caput fafcia verticali lata candida, paululum ante roftrum definente; fafcia atra

* Λευκὸς albus. Ὀφρὺς supercilium.

Lata

lata ad utrumque latus fasciæ albæ. Supercilia alba, desinentia in lineas, fasciam albam verticalem adtingentes; arcus dein atri, ex angulis oculorum, fere in occipite confluentes.

Collum cinerascens, in pectore dilutius.

Dorsum ferrugineo-fuscum, marginibus plumularum cinereis.

Alæ fuscæ; remigum primorum margines exteriores tenuissimi pallidi, interiores cinerascentes: secundarii & pennæ tectrices fuscæ, marginibus latiusculis, versus apicem albis, efficientibus fasciam albam; super quam fascia altera alba ex maculis albis in apice tectricum minorum, s. plumarum scapularium. Alulæ albæ. Remiges subtus cinerei, marginibus albis.

Pectus cinereum, abdomen dilutius, fere album.

Crissum & plumulæ femora tegentes lutescentia.

Uropygium cinereo-fuscum.

Cauda æqualis; rectrices duodecim fuscæ, marginibus paullo pallidioribus, subtus cinereæ.

Pedes carnei coloris, digito intermedio & ungue postico reliquis longioribus.

Longitudo unciarum 7 pedis Anglicani.

Latitudo inter alas extensas 9 unciarum pedis Anglicani.

Cauda partem tertiam longitudinis totius aviculæ efficit.

Alæ complicatæ paulùlum ultra caudæ exortum protenduntur.

Pondus drachmarum sex.

5. FRINGILLA HUDSONIAS.

FRINGILLA fusco-cinerascens, rostro albido, pectore inferiore, abdomine, rectricibusque quatuor extremis albis.

Habitat in America Boreali.

DESCR. *Magnitudo* circiter fringillæ carduelis.

Rostrum albidum, rubedine aliqua imbutum.

Oculi parvi, cœrulei.

Corpus totum cinereo-nigricans, s. potius fuliginosum.

Pectus inferius & *abdomen* alba.

Remiges fusci, cinereo-marginati: alæ complicatæ mediam fere caudam adtingunt.

Rectrices fuscæ, extimæ utrinque duæ totæ albæ, tertia fusca, macula oblonga alba, ad latus interius, prope rachin, apicem attingens; reliquæ totæ fuscæ.

Pondus semunciæ.

Longitudo unciarum 6¼ pedis Anglicani.

Latitudo unciarum novem.

6. MUSCICAPA STRIATA.

MUSCICAPA cinereo-virens, dorso nigro striato, subtus flavescenti-alba, gula lateribusque pectoris fusco maculatis.

Habitat

Habitat ad Sinum Hudſonis.

Quum mas à fœmina multum differat, utique congruum eſt, utrumque ſexum ſeparatim deſcribere.

DESCR. Mas.

Roſtrum trigonum, mandibula ſuperiore paululum longiore, ante apicem leviter emarginata, nigra; inferiore baſi flaveſcente.

Nares ſubrotundæ.

Vibriſſæ nigræ.

Caput ſupra totum atrum ad oculos uſque. *Genæ* à roſtro in occiput totæ albæ; occiput albo & nigro variegatum.

Gula flaveſcenti-alba maculis fuſcis.

Pectus albidum, lateribus, ſive verſus occiput maculis nigris variegatum.

Dorſum cinereo-virens, ſtriis ſive maculis longitudinalibus nigris latioribus, è plumulis nigris, margine virentibus.

Abdomen album.

Uropygium cinereum, nigro-maculatum.

Alæ fuſcæ; remiges primores pallido marginati, ſecundarii apice tenuiſſimo albo; duæ ultimæ margine exteriore albo; tectrices fuſcæ, majores flaveſcenti albo, minores candido in apice maculatæ, unde faſciæ albæ binæ in alis.

Cauda fuſca; rectrix utrinque prima ſ. extima, latere interiore macula magna alba, marginem interiorem attingente; proxima ſ. ſecunda macula oblonga minore alba, etiam marginem interiorem attingente;

attingente; utrinque tertia, latere interiore versus apicem albo-marginata.

Pedes lutei; ungues breves, pallide fusci.

Magnitudo circiter *Pari atricapilli*; Linn.

Longitudo 5 unciarum.

Latitudo 7 unciarum pedis Anglicani.

Fœmina.

Rostrum, alæ, cauda, abdomen, uropygium, pedes & mensuræ ut in mare.

Caput flavo-virens, striis brevibus tenuibusque longitudinalibus nigris; linea flavissima à basi rostri incipiens super oculos ducta; palpebræ flavæ.

Gula, genæ & pectus albido-flava; maculæ sparsæ oblongiusculæ fuscæ, ab utroque oris angulo usque in pectoris latera.

Dorsum, ut in mare, sed viridius, & striæ nigræ minores.

7. Parus Hudsonicus.

Parus capite fusco-rubescente, dorso cinereo, jugulo atro, fascia suboculari, pectoreque albis, hypochondriis rufis.

Habitat ad Sinum Hudsonis.

Descr. *Rostrum* subulatum, integerrimum, atrum, basi è regione narium tectum fasciculis setarum ferruginearum, lineas 4 (unciæ pedis Anglicani) longum.

Caput fusco-ferrugineum, fascia sub oculis alba; gula atra, nigredine extensa sub hac fascia alba.

Dorsum

Dorſum cinereo-virens, è plumis longioribus, fuſcis, apice tantum cinereo-virentibus, ſ. olivaceis.

Pectus & Abdomen alba, ſed plumæ omnes baſi nigræ, apice tantum albæ.

Latera abdominis & lumbi ferruginei.

Alæ fuſcæ, remigum margine omni cinereo.

Cauda fuſca, rotundata, rectricibus 12, margine cinereis.

Uropygium tectum plumulis aliquot nigris, apice albido-rufis.

Pedes nigri; digitus poſticus cum ungue anticorum digitorum medio, duplo longior.

Longitudo unciarum $5\frac{1}{4}$ pedis Anglicani.

Latitudo unciarum 7.

Cauda uncias $2\frac{1}{2}$ longa.

8. Scolopax borealis.

Scolopax roſtro arcuato, pedibuſque nigris, corpore fuſco, griſeo-maculato, ſubtus ochroleuco.

Habitat in Sinus Hudſonis inundatis, & pratis humidis, victitans vermibus & inſectis: menſe Aprili vel initio Maii primum viſa eſt, circa Caſtellum *Albany*, inde in terras magis arcticas migrat, ibique nidificat; redit ad idem caſtellum menſe Auguſto; regiones Auſtraliores petit circa finem Septembris.

Affinis ſcolopace arquata Linn. ſed differt corpore triplo minore, roſtro ratione corporis breviore,

breviore, colore in dorſo ſaturate fuſco, in abdomine ochroleuco.

DESCR. *Caput* pallidum, lineolis conſertis longitudinalibus fuſcis: ſinciput ſaturate fuſcum, pallido maculatum.

Roſtrum nigricans, arcuatum, longitudine duarum unciarum pedis Anglicani, mandibula inferiore baſi rufa.

Collum, pectus, abdomen & criſſum ochroleuca; pectore colloque lineolis longitudinalibus fuſcis conſertioribus, abdomine & criſſo fere nullis, vel tenuibus notatis.

Femora ſemi-tecta plumulis ochroleucis, fuſco maculatis.

Latera abdominis ſub alis præſertim, rufa, pennis tranſverſim fuſco faſciatis.

Dorſum totum ſaturate fuſcum, pennis margine albido griſeis.

Alæ fuſcæ; remiges primores immaculati, primores rachi tota alba; reliqui, ſ. ſecundarii pallide griſeo-marginati. Tectrices late griſeo-marginatæ. Tectrices inferiores alæ, ferrugineæ fuſco tranſverſim faſciatæ. Alæ complicatæ fere mediam caudam attingunt.

Uropygium fuſcum, marginibus maculiſque pennarum albidis.

Cauda brevis, fuſca, rectricibus albido tranſverſim faſciatis

Pedes nigri, ſ. cœruleſcentes.

Longitudo unciarum 13¼.

Latitudo circiter unciarum 21.

9. ANAS NIVALIS.

ANAS, roſtro cylindrico, corpore albo, remigibus primoribus nigris.

Habitat in America Boreali, per Sinum Hudſonis migrans.

DESCR. *Corpus* totum album, magnitudine anſeris domeſtici noſtratis.

Roſtrum luteum, mandibulis ſubſerratis.

Oculi iride rubra.

Remiges decem primores nigri, ſcapis albis: tectrices infimæ cinereæ, ſcapis nigris; pennæ duæ alulæ, itidem cinereæ, ſcapis nigris.

Pedes rubri.

Longitudo pedum duorum & unciarum octo.

Latitudo pedum $3\frac{1}{2}$.

Pondus librarum 5 vel 6.

XXX. *Geometrical Solutions of three celebrated Astronomical Problems, by the late Dr.* Henry Pemberton, *F. R. S. Communicated by* Matthew Raper, *Esq; F. R. S.*

LEMMA.

Read June 4, 1772.

TO form a triangle with two given sides, that the rectangle under the sine of the angle contained by the two given sides, and the tangent of the angle opposite to the lesser of the given sides, shall be the greatest that can be.

Let [TAB. XII. Fig. 1.] the two given sides be equal to A B and A C: round the center A, with the interval A C, describe the circle C D E, and produce B A to E; take B F a mean proportional between B E and B C, and erect the perpendicular F G, and complete the triangle A G B.

Here the sine of B A G is to the radius, as F G to A G; and the tangent of A B G to the radius, as F G to F B: therefore, the rectangle under the sine of B A G and the tangent of A B G is to the square of

the

Fig. 1.

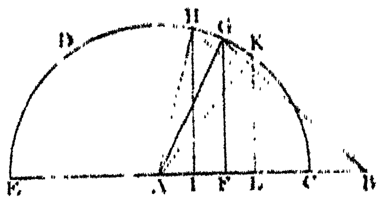


Fig. 2.

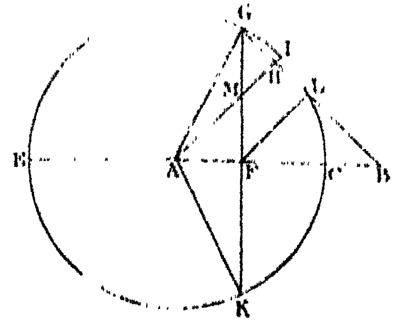


Fig. 3.

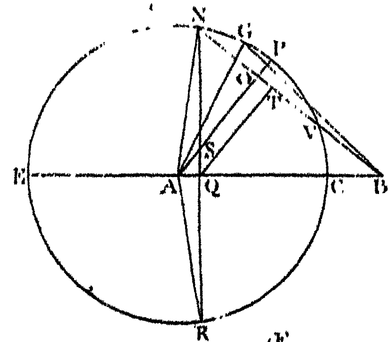


Fig. 4.

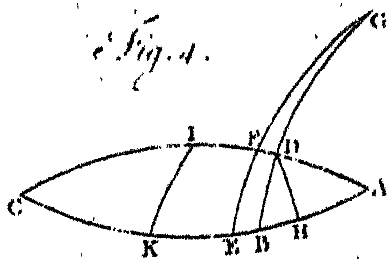


Fig. 5.

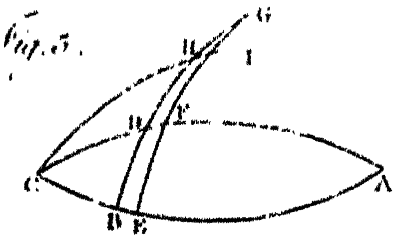


Fig. 6.

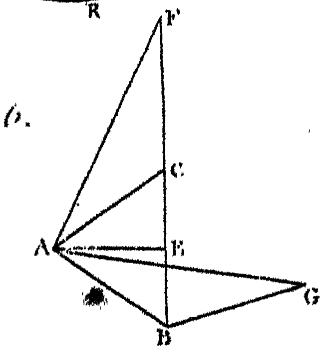
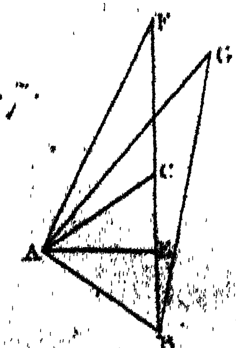


Fig. 7.



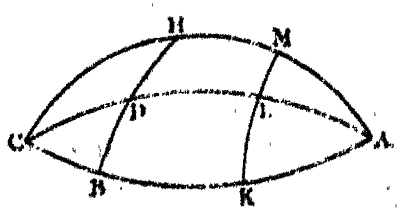


Fig. 8.

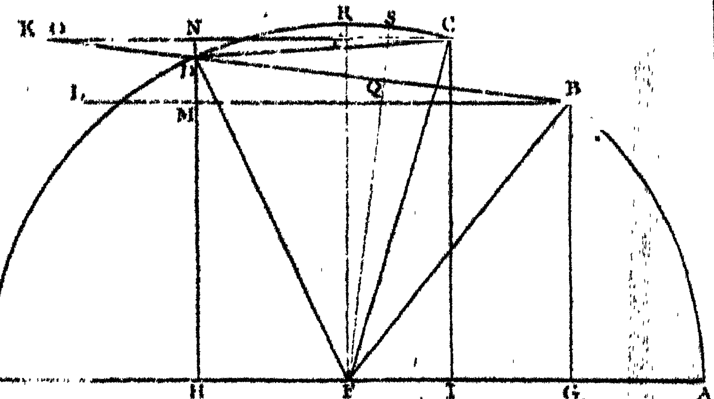


Fig. 9.

Basire sc.

the radius, as the ſquare of FG, or the rectangle EFC, to the rectangle under AG (or AC) and FB, But, EB being to BF as BF to BC, by converſion, EB is to EF as BF to FC, and alſo, by taking the difference of the antecedents and of the conſequents, EF is to twice AF as BF to FC; and twice AFB is equal to EFC.

Now, let the triangle BAH be formed, where the angle BAH is greater than BAG. Here, the perpendicular HI being drawn, the rectangle under the ſine of BAH and the tangent of ABH will be to the ſquare of the radius, as the rectangle EIC to the rectangle under AC, IB. But IF is to FB as 2AFI to 2AFB, or EFC; and 2AFI is greater than $AF^2 - AI^2$; alſo $AF^2 - AI^2$ together with EFC, is equal to EIC; therefore, by compoſition, the ratio of IB to BF is greater than that of EIC to EFC; and the ratio of AC×IB to AC×FB greater than that of EIC to EFC: alſo, by permutation, the ratio of AC×IB to EIC greater than the ratio of AC×FB to EFC. But the firſt of theſe ratios is the ſame with that of the ſquare of the radius to the rectangle under the ſine of BAH and the tangent of ABH; and the latter is the ſame with that of the ſquare of the radius to the rectangle under the ſine of BAG and the tangent of ABG; therefore, the latter of theſe two rectangles is greater than the other.

Again, let the triangle BAK be formed, with the angle BAK leſs than BAG, and the perpendicular KL be drawn. Then the rectangle under the ſine of BAK and the tangent of ABK is to the ſquare of the radius, as the ſquare of KL to the rectangle under

AC, BL. Here, FL being to FB as 2 AFL to 2 AFB or EFC, and 2 AFL less than $AL^2 - AF^2$, by conversion, the ratio of LB to FB will be greater than the ratio of ELC to EFC; therefore, as before, the rectangle under the sine of BAG and the tangent of ABC is greater than that under the sine of BAK and the tangent of ABK.

Corollary 1.

BF is equal to the tangent of the circle from the point B; therefore, BF is the tangent, and AB the secant, to the radius AC, of the angle, whose cosine is to the radius as AC to AB. Therefore, AF is the tangent, to the same radius, of half the complement of that angle; and AF is also the cosine of the angle BAG to this radius.

Corol. 2.

The sine of the angle composed of the complement of AGB, and twice the complement of ABG, is equal to three times the sine of the complement of AGB. Let fall the perpendicular AH (Fig. 2.), cutting the circle in I; continue GF to K, and draw AK. Then $BF^2 = EBC = GBL$. Therefore, GB : BF :: BF : BL, and the triangles GBF, FBL are similar. Consequently FL is perpendicular to GB, and parallel to AH; whence GH being equal to HL, GM is equal to MF, and MK equal to three times GM.

Now, the arc IK = 2 IC + GI; and the angle IAK = 2 IAC + GAI; also GM is to MK as the

the ſine of the arc GI to the ſine of the arc IK, that is, as the ſine of the angle GAI to the ſine of the angle IAK. Therefore, the ſine of the angle IAK ($=2$IAC $+$ GAI) is equal to three times the ſine of the angle GAI; but GAI is the complement of AGB, and IAC the complement of ABG.

COROL. 3.

If (Fig. 3.) any line BN be drawn to divide the angle ABG, and AN be joined, alſo AO be drawn perpendicular to BN, and continued to the circle in P, the ſine of the angle compoſed of NAP and 2PAC will be leſs than three times the ſine of the angle NAP. Draw NQR perpendicular to AB, cutting AP in S; join AR, and draw QT perpendicular to BN, and parallel to AO; then $BQ^q =$ NBT. But BQ^q is greater than the rectangle EBC, that is, greater than the rectangle NBV, under the two ſegments of the line BN drawn from B, to cut the circle in N and V: therefore, TB is greater than VB, and NQ greater than OT. Conſequently, NS is greater than SQ. Hence RS is leſs than three times NS; and therefore, the ſine of the angle PAR ($=$NAP$+2$PAC) is leſs than three times the ſine of NAP.

PROBLEM.

PROBLEM I.

To find in the ecliptic the point of longest ascension.

ANALYSIS.

Let (Fig. 4.) ABC be the equator, ADC the ecliptic, BD the situation of the horizon, when D is the point of longest ascension. Let EFG be another situation of the horizon. Then the ratio of the sine of EB to the sine of FD is compounded of the ratio of the sine of BG to the sine of GD, and of the ratio of the sine of AE to the sine of AF; but the angles B and E being equal, the arcs EG, GB together make a semicircle; and, by the approach of EG towards GB, the ultimate magnitude of BG will be a quadrant, and the ultimate ratio of EB to FD will be compounded of the ratio of the radius to the sine of DG (that is, the cosine of BD) and of the ratio of the sine of AB to the sine of AD. Draw the arc DH perpendicular to AB. Then, in the triangle BDH, the radius is to the cosine of BD, as the tangent of the angle BDH to the cotangent of HBD. Also, in the triangle BDA, the sine of AB is to the sine of AD as the sine of the angle BDA (or BDC) to the sine of ABD; therefore, the ultimate ratio of BE to DF is compounded of the ratio of the tangent of BDH to the cotangent of ABD, and of the ratio of the sine of BDC to the sine of ABD; which two ratios compound that of the rectangle under the tangent of BDH and the sine of BDC to the rectangle under the cotangent and the sine of the given angle ABD.

 But

But, when D is the point of longeſt aſcenſion, the ratio of BE to DF is the greateſt that can be; therefore, then the ratio of the rectangle under the tangent of BDH and the ſine of BDC to the given rectangle under the cotangent and ſine of the given angle ABD muſt be the greateſt that can be; and conſequently, the rectangle under the tangent of BDH, and the ſine of BDC, muſt be the greateſt that can be.

In the triangle BDA, the ſine of BDH is to the ſine of HDA, as the coſine of ABD to the coſine of BAD. Now, in the preceding lemma, let the angle BAG of the triangle AGB be equal to the ſpherical angle BDC: then will the ſum of the angles ABG, AGB be equal to the ſpherical angle BDA. And, if AG in the triangle AGB, be to AB as the coſine of the ſpherical angle DBA to the coſine of DAB, that is, as the ſine of BDH to the ſine of HDA, the angle ABG, in the triangle, will be equal to the ſpherical angle BDH; and the angle AGB, in the triangle, equal to the ſpherical angle HDA. Therefore, by the firſt corollary of the lemma, that the rectangle under the tangent of the ſpherical angle BDH and the ſine of BDC be the greateſt that can be, the coſine of BDC muſt be equal to the tangent of half the complement of the angle, whoſe coſine is to the radius, as AG to AB, in the triangle, or as the coſine of the ſpherical angle ABD to the coſine of the ſpherical angle BAD.

If IK be the ſituation of the horizon, when the ſolſtitial point is aſcending, in the quadrantal triangle AIK, the coſine of KIC is to the radius as the coſine of IKA (= DBA) to the coſine of IAK. Therefore,

fore, the cosine of B D C, when D is the point of longest ascension, is equal to the tangent of half the complement of the angle, which the ecliptic makes with the horizon, when the solstitial point is ascending.

But, the sine of the angle composed of D A B, and twice A B D, must be less than three times the sine of the angle B A D. In the spherical triangle A B D, the angles B A D, A B D together exceed the external angle B D C. Therefore, in the third corollary of the lemma, let the angle B A N be equal to the sum of the spherical angles B A D, A B D: but here, A N is to A B as the cosine of the spherical angle A B D to the cosine of B A D; and A N is also to A B as the sine of A B N to the sine of A N B, that is, as the cosine of B A P to the cosine of N A P; consequently, since the angle B A N is equal to the sum of the spherical angles B A D, A B D, the angle N A P is equal to the spherical angle B A D, and the angle B A P equal to the spherical angle A B D; but the sine of the angle composed of N A P and twice P A B is less than three times the sine of N A P; therefore, the sine of the angle composed of the spherical angle B A D and 2 A B D will be less than three times the sine of the angle B A D; otherwise no such triangle D B A, as is here required, can take place, but the point A will be the point of longest ascension.

If the sine of the angle A be greater than one third of the radius, the point A can never be the point of longest ascension; but when the sine of this angle is less, the angle compounded of B A D and twice A B D, may be greater or less than a quadrant;

and

and therefore, the magnitude of the angle ABD, that A be the point of longeſt aſcenſion, is confined within two limits, of which the double of one added to the angle A, as much exceeds a quadrant, as the double of the other added to that angle falls ſhort of it; therefore, double the ſum of thoſe two angles, together with twice A, makes a ſemicircle; and the ſingle ſum of thoſe two angles added to A makes a quadrant.

PROBLEM II.

To find when the arc of the ecliptic differs moſt from its oblique aſcenſion.

ANALYSIS.

If (Fig. 5.) BD be the ſituation of the horizon, when CD differs moſt from CB, as before, the ultimate ratio of BE to DF will be compounded of the ratio of the radius to the ſine of DG (or the coſine of DB) and of the ratio of the ſine of CB to the ſine of CD: but, when CD differs moſt from CB, BE and DF are ultimately equal; therefore, then the coſine of BD is to the radius as the ſine of CB to the ſine of CD.

Draw the arc CHI of a great circle, that DH be equal to DB; then, BH being double BD, half the ſine of BH is to the ſine of BD or DH, as the coſine of BD to the radius; therefore, half the ſine of BH is to the ſine of DH as the ſine of CB to the ſine of CD; but the ſine of the angle BCH is to the ſine of BH as the ſine of the angle CHB to the

 ſine

ſine of CB; whence, by equality, half the ſine of BCH is to the ſine of DH as the ſine of CHB to the ſine of CD: but as the ſine of CHB to the ſine of CD, ſo, in the triangle CHD, is the ſine of DCH to the ſine of HD: conſequently, the ſine of DCH is equal to half the ſine of BCH. Hence, the difference of the angles BCH, DCH being given, thoſe angles are given, and the arc CH is given by poſition.

Moreover, in the triangle BCH, the baſe BH being biſected by the arc CD, the ſine of the angle CHD is to the ſine of the given angle CBD, as the ſine of the given angle HCD to the ſine of the given angle BCD; therefore, the angle CHB is given; in ſo much, that in the triangle CBH all the angles are given.

The ſum of the ſines of the angles BCH, DCH is to the difference of their ſines, as the tangent of half the ſum of thoſe angles to the tangent of half their difference; therefore, the tangent of half the ſum of BCH, DCH is three times the tangent of half BCD.

In (Fig. 6.) the iſoſceles triangle ABC, let the angle BAC be equal to the ſpherical angle BCD, and let AE be perpendicular to BC; alſo, CF being taken equal to CB, join AF: then EF is equal to three times EB; and as EF to EB, ſo is the tangent of the angle EAF to the tangent of EAB; but EAB is equal to half the ſpherical angle BCD: therefore, the angle EAF is equal to half the ſum of the ſpherical angles BCD, BCH; and conſequently, the angle CAF equal to the ſpherical angle DCH. Here, AF is to CF as the ſine of the angle ACF

 to

to the ſine of C A F; and C B is to A B as the ſine of the angle B A C to the ſine of A C B: therefore, C F being equal to C B, and the ſine of A C F to the ſine of A C B, by equality, A F is to A B as the ſine of the angle B A C to the ſine of C A F, that is, as the ſine of the ſpherical angle B C D to the ſine of the ſpherical angle D C H.

Let (Fig. 7.) the triangle A G B have the angle A B G equal to the ſpherical angle C B D, and the ſide A G equal to A F. Then, A G is to A B as the ſine of the ſpherical angle B C D to the ſine of the ſpherical angle D C H, that is, as the ſine of the ſpherical angle C B H to the ſine of the ſpherical angle C H B: but A G is to A B alſo as the ſine of the angle A B G to the ſine of A G B; therefore, the angle A B G being equal to the ſpherical angle C B H, the angle A G B is equal to the ſpherical angle C H B: and moreover, when the angle A B G is greater than A B F, that is, when the ſpherical angle C B H is greater than the complement of half B C D, the three angles A B G, A G B and B A C together exceed two right.

Hence, (Fig. 8.) towards the equinoctial point C, where the angle C B D is obtuſe, a ſituation of the horizon, as B D, may always be found, wherein C D more exceeds C B than in any other ſituation: and when the acute angle D B A is greater than the complement of half B C D, another ſituation of the horizon, as K L M, may be found, toward the other equinoctial point A, wherein the arc of the ecliptic C K will be leſs than the arc of the equator, and their difference be greater than in any other ſituation. But, if the angle D B A be not greater than the com-

 plement

plement of half BCD, the arc of the ecliptic, between C and the horizon, will never be less than the arc of the equator, between the same point C and the horizon.

In the two situations of the horizon, the angles CHB and KMA are equal.

SCHOLIUM 1.

To find the point in the ecliptic, where the arc of the ecliptic most exceeds the right ascension, is a known problem: that point is, where the cosine of the declination is a mean proportional between the radius and the cosine of the greatest declination.

In the preceding figure, supposing the angle CBD to be right, then, because when CD most exceeds CB, the cosine of BD is to the radius as the sine of CB to the sine of CD, and, in the triangle CBD, the sine of CB is to the sine of CD as the sine of the angle CDB to the radius, also the sine of CDB is to the radius as the cosine of BCD to the cosine of BD; therefore, the cosine of BD is to the radius as the cosine of the angle BCD to the cosine of the same BD, and the cosine of BD is a mean proportional between the radius and the cosine of BCD.

SCHOLIUM 2.

In any given declination of the Sun, to find when the azimuth most exceeds the angle which measures the time from noon, is a problem analogous to the preceding.

Dr.

PROBLEM III.

*The tropic found, by Dr. Halley's method *, without any consideration of the parabola.*

The obſervations are ſuppoſed to give the proportions between the differences of the ſines of three declinations of the Sun near the tropic; but the ſine of the Sun's place is in a given proportion to the ſine of the declination; therefore, the ſame obſervations give equally the proportion between the differences of the ſines of the Sun's place, in each obſervation.

Now (Fig. 9.), let ACE be the ecliptic, AE its diameter between ♈ and ♎, and its center F; let B, C, D be three places of the Sun; BG, CI, DH the ſines of thoſe places reſpectively. Draw CK, BL parallel to AE, which may meet HD, in N and M. Then, by the obſervations, the ratio of DM to DN is given. Therefore, if BD be drawn to meet KL in O, the ratio of BD to OD is given; and the ratio of BD to DC is alſo given, they being the chords of the given angles BFD, CFD: hence the ratio of CD to DO, in the triangle CDO, is given; and conſequently, the angle COD will be given: which angle is the diſtance of the tropic from the middle point of the ecliptic between B and D: for, FPR being perpendicular to OC, and FQS perpendicular to DB, the angle QFP is equal to QOP, the points O, P, Q, F, being in a circle.

* Vide Philoſophical Tranſactions, N° 215.

The Calculation.

$$\left.\begin{array}{l} DN : DM \\ \text{s. } \tfrac{1}{2} BFD : \text{s. } \tfrac{1}{2} CFD \end{array}\right\} :: \text{rad.} : \text{t. } \angle\, \chi$$

$$\text{rad.} : \text{t. } \angle\, \overline{\chi \backsim 45^\circ} :: \text{t. } \tfrac{1}{2} BFC : \text{t. } \frac{COD \backsim DCO}{2}$$

If $\chi > 45^\circ$, $\angle\, COD > DCO$

And

if $\chi < 45^\circ$, $\angle\, COD < DCO$.

If the intervals between the obſervations are ſo ſmall, that the ſines differ not much from the arches, the arches BC, CD may be counted in time, and the calculation may be abbreviated thus:

$$DM : DN :: \text{arc. } BD : Z \text{ (for } DO)$$
$$DC + Z : 2\,DC :: \tfrac{1}{2} BC : SR.$$

Or,

$$DM \times DC + DN \times BD : DM \times DC :: \tfrac{1}{2} BC : SR.$$

XXXI. *On*

Received May 18, 1772.

XXXI. *On the Digeſtion of the Stomach after Death, by* John Hunter, *F. R. S. and Surgeon to* St. George's *Hoſpital.*

Read June 18, 1772.

AN accurate knowledge of the appearances in animal bodies that die of a violent death, that is, in perfect health, or in a ſound ſtate, ought to be conſidered as a neceſſary foundation for judging of the ſtate of the body in thoſe that are diſeaſed.

But as an animal body undergoes changes after death, or when dead, it has never been ſufficiently conſidered what thoſe changes are; and till this be done, it is impoſſible we ſhould judge accurately of the appearances in dead bodies. The diſeaſes which the living body undergoes (mortification excepted) are always connected with the living principle, and are not in the leaſt ſimilar to what may be called diſeaſes or changes in the dead body: without this knowledge, our judgment of the appearances in dead bodies muſt often be very imperfect, or very erroneous; we may ſee appearances which are natural, and may ſuppoſe them to have ariſen from diſeaſe; we may ſee diſeaſed parts, and ſuppoſe them in a natural ſtate; and we may ſuppoſe a circumſtance to have exiſted be-

fore

fore death, which was really a conſequence of it; or we may imagine it to be a natural change after death, when it was truly a diſeaſe of the living body. It is eaſy to ſee therefore, how a man in this ſtate of ignorance muſt blunder, when he comes to connect the appearances in a dead body with the ſymptoms that were obſerved in life; and indeed all the uſefulneſs of opening dead bodies depends upon the judgement and ſagacity with which this ſort of compariſon is made.

There is a caſe of a mixed nature, which cannot be reckoned a proceſs of the living body, nor of the dead; it participates of both, inaſmuch as its cauſe ariſes from the living, yet cannot take effect till after death.

This ſhall be the object of the preſent paper; and, to render the ſubject more intelligible, it will be neceſſary to give ſome general ideas concerning the cauſe and effects.

An animal ſubſtance, when joined with the living principle, cannot undergo any change in its properties but as an animal; this principle always acting and preſerving the ſubſtance, which it inhabits, from diſſolution, and from being changed according to the natural changes, which other ſubſtances, applied to it, undergo.

There are a great many powers in nature, which the living principle does not enable the animal matter, with which it is combined, to reſiſt, viz. the mechanical and moſt of the ſtronger chemical ſolvents. It renders it however capable of reſiſting the powers of fermentation, digeſtion, and perhaps ſeveral others, which are well known to

act

act on this ſame matter, when deprived of the living principle, and entirely to decompoſe it. The number of powers, which thus act differently on the living and dead animal ſubſtance, is not aſcertained: we ſhall take notice of two, which can only affect this ſubſtance when deprived of the living principle; which are, putrefaction and digeſtion. Putrefaction is an effect which ariſes ſpontaneouſly; digeſtion is an effect of another principle acting upon it, and ſhall here be conſidered a little more particularly.

Animals, or parts of animals, poſſeſſed of the living principle, when taken into the ſtomach, are not the leaſt affected by the powers of that viſcus, ſo long as the animal principle remains; thence it is that we find animals of various kinds living in the ſtomach, or even hatched and bred there: but the moment that any of thoſe loſe the living principle, they become ſubject to the digeſtive powers of the ſtomach. If it were poſſible for a man's hand, for example, to be introduced into the ſtomach of a living animal, and kept there for ſome conſiderable time, it would be found, that the diſſolvent powers of the ſtomach could have no effect upon it; but if the ſame hand were ſeparated from the body, and introduced into the ſame ſtomach, we ſhould then find that the ſtomach would immediately act upon it.

Indeed, if this were not the caſe, we ſhould find that the ſtomach itſelf ought to have been made of indigeſtible materials; for, if the living principle was not capable of preſerving animal

 ſubſtances

ſubſtances from undergoing that proceſs, the ſtomach itſelf would be digeſted.

But we find on the contrary, that the ſtomach, which at one inſtant, that is, while poſſeſſed of the living principle, was capable of reſiſting the digeſtive powers which it contained, the next moment, *viz.* when deprived of the living principle, is itſelf capable of being digeſted, either by the digeſtive powers of other ſtomachs, or by the remains of that power which it had of digeſting other things.

From theſe obſervations, we are led to account for an appearance which we find often in the ſtomachs of dead bodies; and at the ſame time they throw a conſiderable light upon the nature of digeſtion. The appearance which has been hinted at, is a diſſolution of the ſtomach at its great extremity; in conſequence of which, there is frequently a conſiderable aperture made in that *viſcus*. The edges of this opening appear to be half diſſolved, very much like that kind of diſſolution which fleſhy parts undergo when half digeſted in a living ſtomach, or when diſſolved by a cauſtic alkali, *viz.* pulpy, tender, and ragged.

In theſe caſes the contents of the ſtomach are generally found looſe in the cavity of the *abdomen*, about the ſpleen and diaphragm. In many ſubjects this digeſtive power extends much further than through the ſtomach. I have often found, that after it had diſſolved the ſtomach at the uſual place, the contents of the ſtomach had come into contact with the ſpleen and diaphragm,

 had

had partly diſſolved the adjacent ſide of the ſpleen, and had diſſolved the diaphragm quite through; ſo that the contents of the ſtomach were found in the cavity of the *thorax*, and had even affected the lungs in a ſmall degree.

There are very few dead bodies, in which the ſtomach is not, at its great end, in ſome degree digeſted; and one who is acquainted with diſſections, can eaſily trace the gradations from the ſmalleſt to the greateſt.

To be ſenſible of this effect, nothing more is neceſſary, than to compare the inner ſurface of the great end of the ſtomach, with any other part of the inner ſurface; what is found, will appear ſoft, ſpongy, and granulated, and without diſtinct blood veſſels, opaque and thick; while the other will appear ſmooth, thin, and more tranſparent; and the veſſels will be ſeen ramifying in its ſubſtance, and upon ſqueezing the blood which they contain from the larger branches to the ſmaller, it will be found to paſs out at the digeſted ends of the veſſels, and appear like drops on the inner ſurface.

Theſe appearances I had often ſeen, and I do ſuppoſe that they had been ſeen by others; but I was at a loſs to account for them; at firſt, I ſuppoſed them to have been produced during life, and was therefore diſpoſed to look upon them as the cauſe of death; but I never found that they had any connection with the ſymptoms: and I was ſtill more at a loſs to account for theſe appearances when I found that they were moſt frequent in thoſe who died of violent deaths, which made

me ſuſpect that the true cauſe was not even imagined*.

At this time I was making many experiments upon digeſtion, on different animals, all of which were killed, at different times, after being fed with different kinds of food; ſome of them were not opened immediately after death, and in ſome of them I found the appearances above deſcribed in the ſtomach. For, purſuing the enquiry about digeſtion, I got the ſtomachs of a vaſt variety of fiſh, which all die of violent deaths, and all may be ſaid to die in perfect health, and with their ſtomach commonly full; in theſe animals we ſee the progreſs of digeſtion moſt diſtinctly; for as they ſwallow their food whole, that is, without maſtication, and ſwallow fiſh that are much larger than

* The firſt time that I had occaſion to obſerve this appearance in ſuch as died of violence and ſuddenly, and in whom therefore I could not eaſily ſuppoſe it to be the effect of diſeaſe in the living body, was in a man who had his ſkull fractured and was killed outright by one blow of a poker. Juſt before this accident, he had been in perfect health, and had taken a hearty ſupper of cold meat, cheeſe, bread, and ale. Upon opening the *abdomen*, I found that the ſtomach, though it ſtill contained a good deal, was diſſolved at its great end, and a conſiderable part of theſe its contents lay looſe in the general cavity of the belly. This appearance puzzled me very much. The ſecond time was at St. George's Hoſpital, in a man who died a few hours after receiving a blow on his head, which fractured his ſkull likewiſe. From thoſe two caſes, among other conjectures about ſo ſtrange an appearance, I began to ſuſpect that it might be peculiar to caſes of fractured ſkulls; and therefore, whenever I had an opportunity, I examined the ſtomach in every perſon who died of that accident: but I found many of them which had not this appearance. Afterwards I met with it in a ſoldier who had been hanged.

the

the digesting part of the stomach can contain (the shape of the fish swallowed being very favourable for this enquiry,) we find in many instances that the part of the swallowed fish which is lodged in the digesting part of the stomach is more or less dissolved, while that part which remains in the *œsophagus* is perfectly sound.

And in many of these I found, that this digesting part of the stomach was itself reduced to the same dissolved state as the digested part of the food.

Being employed upon this subject, and therefore enabled to account more readily for appearances which had any connection with it, and observing that the half-dissolved parts of the stomach, &c. were similar to the half-digested food, it immediately struck me that it was from the process of digestion going on after death, that the stomach, being dead, was no longer capable of resisting the powers of that menstruum, which itself had formed for the digestion of its contents; with this idea, I set about making experiments to produce these appearances at pleasure, which would have taught us how long the animal ought to live after feeding, and how long it should remain after death before it is opened; and above all, to find out the method of producing the greatest digestive power in the living stomach: but this pursuit led me into an unbounded field.

These appearances throw considerable light on the principles of digestion; they shew that it is not mechanical power, nor contractions of the stomach, nor heat, but something secreted in the coats of the stomach,

ſtomach, which is thrown into its cavity, and there animaliſes the food *, or aſſimilates it to the nature of the blood. The power of this juice is confined or limited to certain ſubſtances, eſpecially of the vegetable and animal kingdoms; and although this menſtruum is capable of acting independently of the ſtomach, yet it is obliged to that *viſcus* for its continuance.

* In all the animals, whether carnivorous or not, upon which I made obſervations or experiments to diſcover whether or not there was an acid in the ſtomach, (and I tried this in a great variety,) I conſtantly found that there was an acid, but not a ſtrong one, in the juices contained in that *viſcus* in a natural ſtate.

XXXII. *Experiments and Obſervations on the Waters of* Buxton *and* Matlock, *in* Derbyſhire, *by* Thomas Percival, *of* Mancheſter, *M. D. and F. R. S.*

Read June 25, 1772.

THE water of ſaint Ann's-well is found, by analyſis, to contain calcareous earth, foſſil- alkali, and ſea ſalts; but in very ſmall proportions: for a gallon of the water, when evaporated, yields only twenty three, or twenty four grains of ſediment. It ſtrikes a light green colour with ſyrup of violets, ſuffers no change from an infuſion of galls, from the fixed vegetable alkali, or from the mineral acids; becomes milky with the volatile alkali, and with Saccharum Saturni; and lets fall a precipitate on the addition of a few drops of a ſolution of ſilver, in the nitrous acid. The ſpecific gravity of this water is preciſely equal to that of rain water, when their temperatures are the ſame; but it weighs four grains in a pint lighter, when firſt taken from the ſpring. The heat of the bath is about 82 degrees of Fahrenheit's thermometer; that of Saint Ann's well, as it is a ſmaller body of water, and expoſed to the open air, is ſomewhat leſs. The water is tranſparent, ſparkling, and highly grateful to the palate *.

* I am indebted to the information of the worthy phyſician who attends at Buxton, for ſome of theſe facts.

In

In October 1769, I paſſed a few days at Buxton; and during my ſtay there amuſed myſelf with the following experiments on the effects of the water of Saint Ann's well, on my pulſe.

EXPERIMENT I.

October 12, eight o'clock in the morning. The day cold and moiſt, my pulſe beat 84 ſtrokes in a minute; I drank at the well, the third of a pint of water, and, uſing every neceſſary precaution, examined my pulſe at certain intervals of time; in five minutes, pulſe 80, in ten minutes pulſe 80, fuller and harder; in twenty minutes pulſe 85; in half an hour pulſe 90.

EXPERIMENT II.

Eleven o'clock in the forenoon, two hours after breakfaſt, the air warm and ſerene, pulſe 90; I repeated the draught of water. In ſeven minutes pulſe 109; in fifteen minutes pulſe 103; in thirty minutes pulſe 100, head-ach; in an hour and a half pulſe 95, head-ach abated.

EXPERIMENT III.

October 13, eight in the morning; the day cold, pulſe 92; I drank the quantity of water above-mentioned; in five minutes pulſe 86; in fifteen minutes pulſe 86, full and hard; in twenty minutes pulſe 100; in half an hour pulſe 92.

From the firſt and third experiments, it appears that the coldneſs of the morning counteracted for a time, the effects of the Buxton water; and reduced the

the vibrations of my pulse from 84 to 80, and from 92 to 86. But the stimulus of the water soon became superior to the sedative powers of the cold to which I was exposed; for within the space of half an hour my pulse rose to 90 in the first, and to 100 strokes in the second trial. At eleven o'clock before noon, when the air was warm and serene, the water in a much shorter time excited its force, increasing the velocity of my pulse from 90, to 109 vibrations in a minute. These experiments evince the heating quality of Buxton water, and suggest to us the precautions to be observed in the use of it. Small quantities should only be drunk at once, and frequently repeated; the belly should be kept soluble with lenitive Electuary, or any other mild purgative and at the beginning of the course, the patient may be directed to suffer the water to remain a few seconds in the glass, before he swallows it. For this celebrated spring abounds with a mineral spirit, or mephitic air, in which its stimulus, and indeed its efficacy resides, and which is quickly dissipated by exposure to the air.

The honourable and ingenious Mr. Cavendish has shewn by his Experiments on Rathbone Place water, Ph. Transactions, vol. LVII, that calcareous earths may be rendered soluble in water, by furnishing them with more than their natural property of fixed air. And it has lately been discovered that iron also may be suspended by this principle, in the same menstruum *. It appeared therefore highly probable to me, that a chalybeate impregnation might with great facility

* Vid. Mr. Lane's experiments, Ph. Transactions, Vol. LIX.

be communicated to the Buxton water, when fresh drawn from the spring; a quality, which in many cases would add greatly to its medicinal efficacy. I suggested the trial to Mr. Buxton, a very worthy and sensible apothecary near the wells, who has lately at my request made the following experiment.

Experiment IV.

A quart bottle containing two drachms of iron filings, was filled by immersion, with the water of Saint Anne's well, corked and agitated briskly under the surface of the water: it was then suffered to remain in the well till the filings had subsided, when the water was carefully decanted into a half pint glass; to this were added three drops of the tincture of galls, which immediately occasioned a deep purple colour, and transparency was presently restored by a few drops of the acid of vitriol; evident proofs that a solution of the iron was effected in a few minutes. The water also without the tincture of galls had a chalybeate taste, and left an agreeable astringency on the palate.

By this experiment, it appears that a warm chalybeate abounding with a mineral spirit, and grateful to the taste, may with very little trouble be obtained. And this method of impregnating the Buxton water with iron, must increase its tonic powers, and in many cases improve its medicinal virtues. It is a common practice to join the use of a chalybeate spring in the neighbourhood of St. Anne's well, with that of the Buxton water: but, the superiority of the artificial mineral water must be apparent, if we consider its agreeable warmth, volatility, levity, and gratefulness to the palate.

Buxton bath is very frequently employed as a temperate cold bath. For as the heat of the water is about ſixteen or eighteen degrees below that of the human body, a gentle ſhock is produced on the firſt immerſion, the heart and arteries are made to contract more powerfully, and the whole ſyſtem is braced and invigorated. But this ſalutary operation muſt be greatly diminiſhed, often indeed more than counter balanced, by the relaxing vapours which copiouſly exhale from the bath, to which the patients are expoſed during the time of dreſſing and undreſſing. A ſeparate room is indeed provided for the ladies; but the gentlemen have no other accommodations than what the vault affords in which the bath is contained, and are therefore liable to all the inconveniences ariſing from warmth and moiſture. June 12, 1772, the mercury ſtood in the ſhade at 65, but in this vault quickly aroſe to 78 degrees.

Experiments on MATLOCK WATER.

Experiment I.

A thermometer made by Dollond, and graduated according to Fahrenheit's ſcale, was expoſed for a ſufficient length of time, to the ſteam of the water, as it guſhes from the rock, and alſo immerſed in the baſon that receives it. The mercury roſe to 66 degrees.

Experiment II.

Six drops of Sp. Sal. Ammon. vol. were poured into a glaſs of the ſpring water, which contained

about the ſixth of a pint; a very ſlight cloudineſs immediately enſued, but no precipitation was afterwards obſervable.

EXPERIMENT III.

Six drops of a ſolution of ſalt of tartar occaſioned a cloudineſs, juſt perceptible, in the ſame quantity of water; no precipitation enſued.

EXPERIMENT IV.

Six drops of a ſolution of ſaccharum ſaturni immediately produced a milkineſs in the water, but no ſenſible precipitation.

EXPERIMENT V.

Six drops of a ſolution of ſilver in the nitrous acid inſtantly occaſioned a milkineſs in the water; and after ſtanding an hour, a grey powder was obſervable at the bottom of the glaſs.

EXPERIMENT VI.

Ten drops of the infuſion of galls neither produced any change of colour in the water at the time they were added, nor was the ſlighteſt purple hue perceptible two hours afterwards.

EXPERIMENT VII.

A piece of paper beſmeared with ſyrup of violets was dipped into a glaſs full of water; no change of colour enſued.

EXPERIMENT VIII.

Another piece of paper, moiſtened in the ſame manner with the ſyrup, was placed over a glaſs of water, as ſoon as it was taken from the ſpring. The paper ſuffered no change of colour, although it remained an hour upon the glaſs.

EXPRRIMENT IX.

My pulſe beat 84 ſtrokes in a minute, at the time when I drank a half pint glaſs of the Matlock water; in 20 minutes my pulſe roſe to 86; in half an hour after they ſunk to 82, and continued to vibrate the ſame number of times for an hour, which was as long as I thought it was neceſſary to examine them.

EXPERIMENT X.

The mercury in the thermometer, when immerſed in each of the baths, ſtood at 68: in the river Derwent, which flows through the valley of Matlock, at 52. Theſe experiments were made in the month of June 1772, and the weather was warm.

EXPERIMENT XI.

A four ounce phial, after being accurately counterpoiſed in a very nice balance, was filled to the brim with diſtilled water, which weighed three ounces, four drachms, forty five grains and a half. The ſame phial, exactly balanced as before, was then filled to the brim with Matlock water, of the ſame temperature

perature with the diſtilled water, which weighed three ounces, four drachms, and forty ſix grains.

Matlock water is grateful to the palate, and of an agreeable temperature, but exhibits no marks of any mineral ſpirit, either by its taſte, ſparkling appearance in the glaſs, or by the chemical teſt employed in experiment 8. The ſecond and third experiments ſhew that it is very ſlightly impregnated with Selenites or other earthly ſalts; and of this its comparative levity affords alſo a further proof: for it weighs twenty-ſix grains in a pint lighter than the Mancheſter pump water*, and only four grains heavier than diſtilled water. The precipitation of a grey powder, by the adding of a ſolution of ſilver in aqua fortis to the water, renders it probable that a ſmall portion of ſea ſalt is contained in it. For the powder is found to conſiſt of the particles of ſilver, combined with the muriatic acid, which is ſeparated from the foſſil alkali by the ſuperior affinity the nitrous acid bears to it; and thus a double elective attraction takes place in this experiment.

This water is ſaid to contain iron, but the aſſertion is at leaſt rendered doubtful by the 6th experiment, which was made with the utmoſt accuracy; and I am inclined to think, that it is entirely without foundation. The ſpring is juſtly celebrated for its efficacy in hæmoptoes; and hence it may have been too haſtily concluded that it poſſeſſes ſome ſlight degree of ſtypticity, by means of a chalybeate impregnation.

* Vid. the author's treatiſe on the pump water of Mancheſter. Eſſays medical and experimental, p. 207. 2d edit.

The

The 9th experiment, which my ſhort ſtay at Matlock would not allow me leiſure to repeat, affords a preſumption that the water is not poſſeſſed of any ſtimulating powers; for the ſmall increaſe of quickneſs in my pulſe, on drinking half a pint of it, may be aſcribed more to the quantity received into the ſtomach, than to the heating quality of the water,

The Briſtol and Matlock waters appear to reſemble each other, both in their chemical and medicinal qualities. I have examined and compared them together by the teſt mentioned above, and ſo far as ſuch trials may be be deemed concluſive, there ſeems to be no other than the following ſlight difference between them,

Briſtol water becomes a little more milky on the addition of a ſolution of fixed alkali, and of Saccharum Saturni than that of Matlock; the former alſo weighs near a grain in a pint heavier than the latter. Is it not to be lamented therefore, that ſo little attention is paid to Matlock, even by the phyſicians who reſide in the neighbourhood of it? In hectic caſes, hæmoptoes, the diabetes, and other diſorders, in which the circulation of the blood is rapid and irregular, I ſhould apprehend that Matlock water, on ſome accounts, claims the preſerence to that of Briſtol; for it is leſs diſpoſed to quicken the pulſe, and may therefore be drunk in larger quantities. But it muſt be acknowledged that the climate of Briſtol is ſuperior to that of Matlock, a circumſtance of the higheſt importance to conſumptive patients. Situated in a deep though delightful valley, and ſurrounded by very high mountains, the ſun diſappears

at Matlock earlier in the evenings, the fogs are longer in difperfing, and it may be prefumed that rain falls here more frequently and copioufly than in other places. For at Catfworth, which is encompaffed alfo with hills, and is about ten miles diftant, in 1764, 1765, 1767, and 1768, about 33 inches of rain fell at a medium each year.

The following table exhibits a comparative view of the different temperatures of Bath, Buxton, Briftol, and Matlock waters, meafured by Fahrenheit's thermometer.

* BATH.	
King's Bath Pump	112 °
Hot Bath Pump	114½
Crofs Bath Pump	110
* BRISTOL.	
Hot Well Pump	76
BUXTON.	
Bath	82
St. Ann's Well	81 ×
MATLOCK.	
Baths	68
Spring	66

* Vid. Mr. Canton's experiments. Ph. Tranf. Vol. LVII. p. 203.

XXXIII. *Som*

XXXIII. *Some Account of a Body lately found in uncommon Preservation, under the Ruins of the Abbey, at* St. Edmund's-Bury, Suffolk; *with some Reflections upon the Subject: By* Charles Collignon, *M. D. F. R. S. and Professor of Anatomy at* Cambridge.

Read June 25, 1772.

IN the month of February last, some workmen, digging among the ruins of the above abbey, discovered a leaden coffin, supposed, from some circumstances, to contain the remains of Thomas Beaufort, Duke of Exeter, uncle to king Henry the Fifth. As it certainly was buried before the dissolution of the abbey, it must have been there between two and three hundred years. It was found near the wall, on the left-hand side of the choir of the chapel of the blessed Virgin; not inclosed in a vault, but covered over with the common earth. Upon examining the appearance of the body, the following circumstances were remarkable, as communicated to me, by an ingenious surgeon, on the spot, Mr. Thomas Cullum.

" The body was inclosed in a leaden coffin, surrounding it very close, so that you might easily distin-

guiſh the head and feet. The corpſe was wrapped round with two or three large layers of cere-cloth, ſo exactly applied to the parts, that the piece, which covered the face, retained the exact impreſſion of the eyes and noſe. The dura mater was entire. The brain was of a dark aſh colour, with ſome remaining appearance of the medullary part. The coats of the eye were ſtill whole, and had not totally loſt their gliſtening appearance. There was about half a pint of a bloody-black water in the thorax; and a maſs that ſeemed to be part of the lungs. The pericardium and diaphragm were quite entire. The abdominal viſcera had been taken out very clean, and the integuments and muſcles ſtuck very cloſe to the vertebræ of the back. This cavity looked freſher than that of the thorax. I cut into the pſoas magnus, where there were evident marks of red muſcular fibres. The other muſcles had loſt all their red colour, and were become of a dark brown. The tendons were ſtill ſtrong, and retained their natural appearance. The hands, which are preſerved in ſpirits, retain the nails. There were ſome very ſmall holes in the coffin, out of which had run ſome bloody water, of an offenſive ſmell. All the principal blood-veſſels muſt have been cut through, in taking out the abdominal viſcera: and if no ligature was made upon the veſſels, their contents would eſcape, particularly as aſſiſted by the preſſure of the cere-cloth, which is of conſiderable weight, and, doubtleſs, put on hot. This fluid running out of the coffin, upon its being moved, might occaſion the ſuſpicion of the body being put in pickle."

Thus far Mr. Cullum's account, by which it appears, that the viſcera of the abdomen had been taken out, ſo that the greateſt part of the blood, he obſerves, did probably flow out, during that operation, from the mouths of the divided veſſels, and whoſe diameter is conſiderable. This would greatly reduce the quantity of the fluids. The holes in the coffin, if purpoſely made, would ſeem deſigned to let out extravaſated or tranſuding fluids; but are irreconcileable with the notion of the body being in pickle. If the holes were accidental, the notion of a pickle may ſtill be allowed. Might not the cere-cloth, impregnated, perhaps, with gums or reſins, and, from its taking ſo exact an impreſſion, moſt probably laid on hot preclude the external air; and, if done immediately after the party's death, obviate the depoſition of eggs, or incapacitate them from ever hatching? The lead graſping cloſe, would co-operate with the cere-cloth in the excluſion of air and inſects.

We have undoubted accounts of bodies found very little changed, after long interment, where there was no appearance of any art having been uſed. And there is no doubt ſome conſtitutions are more prone to putrefaction after death than others; theſe circumſtances may be dependant on the age, ſex, and laſt diſeaſe; to which prediſpoſing cauſes, thus attending perſons to the grave, are to be added the ſoil and ſituation in which they are depoſited. Could we be maſters of all theſe particulars, in the few dead bodies hitherto diſcovered greatly free from the uſual putrefaction, it would lead, perhaps, to the probable

caufe of the phænomenon, and point out a proper method of imitation. And till that is done, it is difficult to know how much merit is to be affigned to the art or myftery of embalming, and how much to the power of natural caufes.

XXXIV. *A Letter from* Richard Pulteney, *M. D. F. R. S. to* William Watſon, *M. D. F. R. S. concerning the medicinal Effects of a poiſonous Plant exhibited inſtead of the Water Parſnep.*

DEAR SIR,

Read July 9, 1772.

SOME circumſtances having lately come to my knowledge, relating to the effects of a poiſonous plant, I thought them rather too remarkable not to merit further notice; and, I addreſs them to you with the more propriety, as you have already laid before the publick ſome obſervations * concerning the deleterious qualities of the plant in queſtion, which holds a diſtinguiſhed place among the poiſonous ones that are indigenous in Britain.

Mr. H——n, an attorney of this place, now upwards of forty, at the age of fifteen, began to be affected (after taking cold upon violent exerciſe, as he thinks) with what is uſually called a ſcorbutick diſorder; which ſhewed itſelf more particularly on the outſides of his arms, about the elbows, and on the

* See Philoſophical Tranſactions, Vol. XLIV. p. 227. and Vol. L. p. 856.

the outſides of his legs, from the knees to the ancles, as well as in blotches upon other parts of his body. It had the appearance of a dry branny ſcab or ſcurf, which every night fell off, more or leſs, in ſcales, as is uſual in leprous caſes. At times it puſhed out more than uſual, and thickened the integuments of the limbs conſiderably, after which the ſeparation of ſcales would become very abundant.

For ſeveral years paſt he had been trying a variety of things commonly recommended in ſuch caſes, particularly the quack medicine known by the name of Marcdant's Drops, which he continued for near a twelvemonth, without finding the leaſt ſenſible relief: alſo an electuary of Flos ſulphuris and Cremor tartari, which he had perſevered in for near three years, without finding any other alteration, than that of its preventing coſtiveneſs, to which he was habitually ſubject.

In the winter 1770, this diſorder increaſed upon him very rapidly, without being able to aſſign any reaſon, from any accident that had happened to him, or from any irregularity of his own in point of regimen, in which he was always very exact. At this time, beſides the farther ſpreading of the eruption itſelf, the integuments of the legs thickened very much, and the limbs ſwelled to ſuch a degree, as to render him unable to walk. The quantity of branny ſcurf and ſcales thrown off, at this time, was very great; he ſays "handfuls might have been taken out of his bed every morning."

In this unhappy ſituation, even loathſome to himſelf, it was recommended to him to take the juice of water parſnep, in the quantity of one common table-

ſpoonful

ſpoonful every morning, faſting, mixed with two ſpoonfuls of white mountain wine.

Accordingly, about the middle of January 1771, he procured a half-pint phial of what was ſo called, by means of the perſon who had recommended it, and who had aſſured him that he had been greatly relieved, in a ſimilar diſorder, by it.

The firſt ſpoonful he took did not begin to give any great uneaſineſs for two hours, but after that time, his head began to be affected in a very extraordinary manner; a violent ſickneſs ſoon ſucceeded, and violent vomiting; and, after he was put to bed, there came on cold ſweats, and a very ſtrong and long-continued rigor, ſo that the people about him thought him dying for ſome time; but, in a few hours, all theſe ſymptoms wore off.

Such, however, had been the inveteracy of his diſorder, and ſo ſtrong his deſire to find relief, that he determined not to deſiſt; and, after having omitted his medicine for one day, he repeated it, in nearly the ſame doſe, and with ſimilar effects as to ſickneſs and vomiting, though the uncommon ſenſation in his head, and the ſucceeding rigor, were by no means ſo violent. He had reſolution enough to continue this doſe every other morning, for more than a fortnight, and then reduced it to three tea-ſpoonfulls which was juſt the half of the firſt doſe.

Before he had taken this juice one month, he was ſenſible of a very great change for the better; encouraged, therefore, by theſe appearances he perſevered in its uſe until the middle of April, by which time his ſkin, though not quite cleared, yet had ceaſed to throw off any more ſcurf, was be-

come

come ſoſt, clean, and well conditioned, and, as he has repeatedly aſſured me, he got then into a much better conditioned ſtate, then he had experienced for many years before.

From firſt to laſt, this juice never purged him; though he ſays, even in its reduced doſe, it never failed to occaſion a dizzneſs of the head, a nauſea, and ſickneſs, which were not infrequently ſucceeded by a vomiting, that always inſtantly relieved his head.

From the middle of April to the middle of June, he deſiſted from the uſe of the juice, but, in its ſtead, drank every morning for breakfaſt, the infuſion of the leaves of the ſame plant, which, he ſays, is like common bohea tea. The infuſion ſeldom occaſioned nauſea, or ſickneſs, but always brought on a ſmall degree of vertigo, and in a ſlight manner produced the effects of intoxication from liquor.

In June he went to Harrowgate, as he had deſigned in the ſummer before. Upon firſt drinking and bathing there, he thought himſelf worſe; and his eruptions, having gradually increaſed during the two months that he ſtaid in that place, he was convinced that thoſe waters were of no real ſervice to him. On his coming home, he returned to the uſe of the infuſion, and he aſſures me, that he again found, even by that weak preparation, a very ſpeedy alteration for the better. From that time, he continued it ever ſince, until his ſtock of the herb was exhauſted; his ſkin is now ſo very little affected, that he has but here and there, upon his arms and legs, a very ſmall appearance of his diſorder.

Upon queſtioning him relating to the ſenſible qualities of this medicine, he ſays again, that he

part-

particularly remembers that it never once purged him; not even the firſt doſe, which had ſo nearly poiſoned him. He does not think that it increaſed the ſenſible perſpiration, but is convinced that it was diuretick; and adds, that he thinks it occaſioned, beſides the increaſed flow of urine, a copious ſediment in it, and which he believes was always wanting before.

This is the plain, narrative of the fact. He has aſſured me that no medicine or regimen, among the great variety that he has tried, ever had any ſenſible effect upon his diſorder before; and that nothing but the very early and ſenſible relief he experienced from this juice, could have induced him to perſevere in its uſe, under ſuch uneaſy feelings, as it never failed to produce. Indeed, he makes nothing of the lighter effects of the infuſion, from which, however, he thinks, he has likewiſe reaped no ſmall benefit.

This caſe, the nature and inveteracy of his diſorder, being well known among his neighbours, was much talked of, and raiſed the curioſity of many people. When I firſt heard of it, and was informed of the ſmallneſs of the doſe, and its virulent operation, I could ſcarce doubt that the juice of ſome other plant had been adminiſtered inſtead of that of the water parſnep, which we know to be a ſafe and harmleſs vegetable; medical writers having directed its juice to be drunk, even to the quantity of four ounces for a doſe: and as I know, the *Oenanthe crocata*, hemlock dropwort, to be exceedingly plentiful in this country, ſo much, as to be more eaſily procured than the water parſnep itſelf; I thought it

 probable

probable that that plant had been used in its stead. Upon getting a specimen, it appeared that this had been indeed the case; as also, upon farther enquiry, that it was the juice of the root only, and not of the leaves and stalks, that had been administered. I might here observe, that the expression from the root is not to be depended upon after the plant is advanced towards its flowering state, as the root then becomes light, spungy, and almost destitute of juice.

If you judge this case not improper to be laid before the Royal Society, you will do me the honour of presenting it. Mr. H——n himself is so much convinced of the efficacy of the medicine, that he is desirous of its being known to the world.

I do not enter into any reasoning on this occurrence; I relate it only as a fact, and desire it may have no more weight than every judicious physician knows is due to a single instance. How far it may be proper to give this juice a farther trial, I will not take upon me to determine; but must, as an encouragement to any who may chuse to venture upon it, inform them, that it has not on all persons so much power in producing nausea and sickness, as in the case here before us. I am,

S I R,

with great esteem,

Your obliged humble servant,

Blandford,
March 12, 1774.

R. Pulteney.

P. S. Mr H——— is desirous that it should be known, that he "tried very fruitlessly, among other methods, the drinking of tar-water and sea-water, of each of which, he says, he did not drink less than an hogshead."

XXXV. April 21, 1772. *Experiments on two Dipping-Needles, which Dipping-Needles were made agreeable to a Plan of the Reverend Mr.* Mitchell, *F. R. S. Rector of* Thornhill *in* Yorkshire, *and executed for the Board of Longitude, by Mr.* Edward Nairne, *of* Cornhill, London.

Read July 9, 1772.

THE magnetic needles were twelve inches long, and their axes (the ends of which were of gold allayed with copper) rested on friction-wheels of four inches diameter, each end on two friction-wheels, which wheels were balanced with great care. The ends of the axes of the friction-wheels were likewise of gold allayed with copper, and moved in small holes made in bell-metal; and opposite the ends of the axes of the needles, and the friction-wheels, were flat agates, finely polished. Each magnetic needle vibrated in a circle of bell-metal, divided into degrees and half-degrees, and a line passing through the middle of the needle to the ends pointed to the divisions. The minutes set down in the experiments were, by estimation, as the third of half a degree is counted ten minutes. The instruments were carefully placed, so that the needles vibrated exactly in the magnetic meridian.

meridian. The two needles were nearly balanced before they were made magnetical; but, by a curious contrivance of the Reverend Mr. Mitchell of a crofs fixed on the axes of the needles (on the arms of which were cut very fine fcrews, to receive fmall buttons, that might be fcrewed nearer or farther from the axis), the needles could be adjufted both ways, to a great nicety, after they were made magnetical, by reverfing the poles, and changing the fides of the needle.

First fet of experiments made by Edward Nairne, at his houfe, N° 20, Cornhill.

72 20
72 20
72 20
72 20
72 20
72 20.

Second fet of experiments, with that fide of the inftrument to the Eaft, which was to the Weft in the firft obfervation.

72 10
72 15
72 45 } Here the ends of the axis touched the
72 45 } agates.
72 5
72.

Third

Third ſet of experiments, in which the poles of the needle were reverſed, but the ſame ſide of the inſtrument to the Eaſt, as in the ſecond ſet of experiments, and the needle rather more magnetical, being touched with a larger ſet of magnets.

72 30
72 30
72 30
72 30
72 30
72 30.

Fourth ſet of experiments, viz. the ſame ſide of the inſtrument to the Eaſt, as in the firſt ſet of experiments.

72 10
72 10
72 15 Obſerved by Mr. Wales.
72 10
72 10
72 10.

Fifth experiment, viz. the ſame end of the needle made North, as in the firſt ſet of experiments, and alſo the ſame ſide of the inſtrument to the Weſt, as in the firſt ſet of experiments.

72 20.

Experiments

Experiments made April 22, 1772, with the other Dipping-needle, the inſtrument being put in the ſame place, and with great care, in the magnetic meridian, the needle pointed as under.

72	15	
72	10	The poles of the needle changed.
72	20	The ſide of the inſtrument to the Eaſt, which in the firſt obſervation was to the Weſt.

Leſt any thing magnetical ſhould have affected the needle in Mr. Nairne's houſe, he took this inſtrument, and placed it in the middle of a large room belonging to the London Aſſurance in Birchin-Lane, and then the needle pointed to

72	10 or 15	
72	20	
72	30	The poles of the needle changed.
72	10	The ſide of the inſtrument to the Eaſt, which in the firſt obſervation was to the Weſt.

The dipping-needle brought back to Mr. Edward Nairne's, and put in the ſame place as before, ſtood at

72 10 +

The

In the foregoing experiments, the needle was raised to an horizontal position, and left to vibrate. It was between 8 or 9 minutes before the vibration ceased.

The needle brought to an horizontal position, and one grain and a half laid on the extremity of the South end, was not sufficient to keep it in an horizontal position; but the North end pointed to 35° 30′. One grain and three quarters laid on the extremity of the South end of the needle, was more than sufficient to keep it in an horizontal position, the South end then pointing 6° 45′ below 0.

It having been judged proper to have a Drawing of the Dipping-Needle, the following Plate [TAB. XIII.] has been made, wherein

A A Represents the needle.
B B The ends of the axis resting on the friction-wheels.
CCCC The four friction-wheels.
DDD Where flat agate caps are set in.
E E E The divided circle of bell-metal.
F F F F The ends of the cross for adjusting the needle.
GG Two levels, whereby the line of 0 degrees of the instrument is set horizontal.
H The perpendicular axis, whereby the instrument may be turned, that the divided face of the circle may front the East or West.
I An index fixed to the perpendicular axis H, and which points to an opposite line on the horizontal plate K, when the instrument is turned half round.
LLLL Four adjusting screws to set the instrument horizontal. One of them is hid behind the circle.
MMMM Screws which hold on the glass covers, to keep the needle from being disturbed by the wind.

INDEX

AN

INDEX

TO THE

Sixty-Second VOLUME

OF THE

Philosophical Transactions.

A.

ACETOUS fermentation, its effects, p. 244.
Achard, Mr. his letter on swallows found annually torpid in the Rhine, p. 297.
Acids do not assist in curing air spoiled by putrefaction, p. 202.
Adanson, Mr. his account of European swallows caught near the African coast, examined, p. 277. His mistake about Canary birds, p. 278. His inaccuracy about the Roller, p. 321.
Air artificial, observations upon different kinds of it, p. 147, 148. No kind a conductor of electricity, p. 175. See *Fixed*, *Inflammable*, *Nitrous*.

I N D E X.

Air diminished by a mixture of iron filings and brimstone, p. 207, 208. Very noxious to animals, p. 209.

Air infected with respiration, p. 181. Unsuccessful trials to restore it, p. 183, 184, &c. Is the same with air tainted with animal putrefaction, p. 186, 187. Different from, though analogous to, fixed air, p. 188, 189. Not fatal to several insects, p. 192. Cured by vegetation, p. 193, &c.; and probably by a mixture of fixed air, p. 204.

Air in Ireland, and likewise in England, observed to be in a constant state of positive electricity, during winter, p. 138. Probably by the effect of cold, p. 139.

Air tainted with the fumes of charcoal, p. 225. Extinguishes flame, and destroys animals, p. 227.

Air vitiated by flame, p. 162. How much diminished by it, p. 163. Not altered in its specific gravity, p. 164. Not fatal to animals, p. 165. Whether restored by cold, ibid. Is so by vegetation, p. 166, 167, &c.

Angular distance between two near land objects; how observed by Hadley's quadrant, p. 119, 120.

Animal living, not dissolved in the stomach of another animal, p. 449.

Antipodes our, may have a contrary electricity in the air, p. 189.

Antium, famous for its worship of the goddess Fortune, p. 63.

Aristotle, the author of the opinion about the cuckows having no nest of their own, p. 322. Did not write from his own observations, p. 323.

Ascension, the point of the longest in the ecliptic, found, p. 438.

Astronomical observations at Portsmouth, p. 36, &c.

Astronomical problems solved by Dr. Pemberton, p. 434.

Atmosphere injured by the respiration of animals and putrefaction; probably restored by vegetation, p. 198.

Atmospherical electricity. See *Air*, *Fogs*, *Electricity*.

B.

Babelmandel, ſtreight of, how diſtinguiſhed, p. 80, 81.
Badenach, Dr. James, his deſcription of a bird from Malacca, p. 1.
Barker, Mr. Thomas, his meteorological regiſter, p. 42, 43, &c.
Barrington, Hon. Daines, inveſtigation of the ſpecific characters of the rabbit, and the hare, p. 4. On the periodical appearing or diſappearing of certain birds, p. 265.
Beaufort, duke, uncle to Henry V. his body found laſt year, p. 465.
Belon, his account of quails found at ſea, p. 270. No argument for their migration, p. 271.
Birds, their periodical migration acroſs conſiderable extents of ſea, called in queſtion, p. 266. Objections againſt this opinion, p. 267. At what height they can riſe, p. 268. Whether night is a proper time for their flight, p. 269. Would want food, p. 292. Always fly againſt the wind, p. 293. Their diſappearing during winter, accounted for, p. 300, 301. And during ſummer, p. 306.
Bladders not ſufficient to ſecure different kinds of factitious air, p.
Body, well preſerved 2 or 300 years after death, deſcribed, p. 466, &c.
Bohemian chatterer, a bird, why only ſeen now and then? p. 315.
Borlaſe, Dr. William, his meteorological obſervations for 1771, p. 365.
Bradley, the late Dr. James, a paper of his, containing directions for uſing the common micrometer, p. 46.
Buffon, M. attempts to prove the hare and rabbit to be really diſtinct ſpecies, from their not breeding together,

p. 9. Affirms the fame of wolves and dogs, p. 8. Uncertainty of these trials, p. 9. His opinion of quails leaving Europe during winter, examined, p. 272, 273. Thinks that one species of swallow is migratory, p. 282. Mistakes the martin for the swallow, p. 283. His experiment on the torpidity of a swallow fallacious, p. 284.

Buxton waters analysed and examined, p. 455, 456.

C.

Calcination of metals, its effects upon air, p. 228.

Call, John, Esq. on an Indian sketch of the signs of the Zodiac, p. 353.

Cascalote, a plant employed in California to dye in the deepest and most lasting black, p. 58.

Castle Loed, in the county of Ross, a strong sulphureous water found there, p. 15. Described by Dr. Mackenzie p. 16, 17. Analysed by Dr. Monro, p. 18, 19, &c. Mixed with sea-water, becomes similar to that of Harrowgate, p. 24.

Charcoal, its fumes infect common air, p. 225, 226.

Chart of the Red Sea, by Capt. Newland, p. 77.

Clouds, the nature and degree of their electricity ascertained, p. 142, 143.

Clyster of fixed air administered in a putrid fever, p. 255.

Collignon, professor Charles, on a body found 2 or 300 years after death, p. 405.

Collinson, Mr. his account of swallows found at sea examined, p. 270. Relies too much upon Mr. Adanson's observations, p. 279.

Cook, Capt. John, his account of the flowing of the tides in the South Sea, p. 357.

Cross-bills, grown more common since the plantation of firs, p. 316. Whether they feed on the kernels of apples, p. 317.

Cuckow,

Cuckow, the common opinion about its neftlings doubtful, p. 299. 322. 324, &c. Never migrates from this island, p. 304.

D.

Denarius, of the Plætorian family, defcribed, p. 60.
Dipping-needle a new, defcribed, p. 476. 480.
Dog, breeds with a wolf, p. 9.
Dollond, Mr. Peter, his improvement of Hadley's Quadrant, p. 95, &c.
Dyes in red and yellow, p. 56.

E.

Ecliptic, the point of the longeft afcenfion in it found, p. 438. The greateft difference of the arc from its oblique afcenfion, p. 444.
Elder, ferviceable in preferving plants and trees from infects and flies, p. 348.
Electricity, the theory of it confirmed by a late violent lightning, p. 134, 135. Of the air, fogs, and clouds, p. 138. 145.
Electrometer a new, invented by Mr. Henley, p. 360. Its advantages, p. 361.
Eratofthenes, his fieve miftaken, p. 329. Ill explained, p. 330. Retrieved, p. 332.
Ether imbibes fixed air, p. 156.

F.

Fairnburn water in Scotland analyfed, by Dr. Monro, p. 25.
Fermenting liquors emit a great quantity of fixed air, p. 148, 149. Recovered when flat by a mixture of it, p. 154.

p. 154. Contract a bad ſmell by a reabſorption of fixed air incorporated with ether, p. 156.

Fieldfare, where they breed, p. 313, 314.

Fixed air how produced, p. 148. Its effects upon fermenting liquors, p. 149. Does not inſtantly mix with common air, p. 150. How incorporated with water, p. 151, 152. May be of the nature of an acid, p. 153. Is not abſorbed by ice, p. 154. Fatal to animals and vegetables, p. 157. Cannot ſufficiently be retained in a bladder, p. 158. May be rendered immiſcible with water, p. 160, 161. Tends to correct putrid air, p. 204, 205. Serviceable in putrid diſorders, p. 206. 257.

Flounders have their mouths turned different ways, p. 306.

Fogs always occaſion a poſitive electricity in the air, p. 139. Attended with the ſmell of an excited glaſs tube, p. 140. How their influence on electrical balls may be meaſured, ibid. and p. 145, 146.

Forſter, Mr. John Reinhold, his account of the roots uſed by the Indians at Hudſon's bay to dye porcupine quills, p. 54. His account of ſeveral quadrupeds from Hudſon's bay, p. 370. And of birds from the ſame place, p. 382. His obſervation on the pectinated toes of ſeveral ſpecies of the grous kind, p. 397. His Latin deſcriptions of ſome ſcarce birds from Hudſon's bay, p. 423.

Fortune, ſee *Sors*.

Frankbyn, Dr. Benjamin, his thoughts on the vegetable creation, p. 199. On the attraction of fire by plants, p. 234.

Freſh water, manner of diſtilling it from ſalt water at ſea, p. 90.

G.

Geeſe wild, the higheſt fliers of all birds, p. 268.

George

George Island, its latitude and longitude, by Capt. Wallis, p. 34. Its longitude determined by Mr. Lexell, p. 73, 74.

Glass broken by electricity marked with beautiful colors, p. 363.

Graham, Mr. his remarks on several quadrupeds and birds found at Hudson's bay, p. 370.

Grouses, their genus, may be divided by the form of their toes, p. 297.

Gullet, Mr. Christopher, on the effects of elder upon insects, p. 348.

Guns, heard at a vast distance on the Red Sea, by the pilots at Judda, p. 85.

H.

Hadley's Quadrant, improvements made in it by Mr. Dollond, p. 95, 96. And by Mr. Maskeline, p. 99.

Hare Alpine, described, p. 11. 375.

Hare, the genus not easily distinguished from that of the rabbit, p. 4. Mistakes of authors in attempting to settle proper criteria between them, p. 5, 6. Two new characters proposed, p. 10.

Hare, from Hudson's bay, is one third less than the European hare, p. 5. Its different cloathing at different times of the year, p. 12. Manner of this change, p. 13. Some particulars of his way of living, p. 14—376.

Henley, Mr. William, his account of the lightning which fell on the chapel of Tottenham-Court-Road, p. 131, 132, &c. His new electrometer, p. 259. His experiments on breaking glass by means of electricity, p. 362.

Hey, Mr. his experiments to prove that there is no oil of vitriol in water impregnated with fixed air, p. 2[illegible]3.

Hird, J. Z. Esq; his account of a new species of oak, p. 1[illegible]8.

Horsley, Mr. on Eratosthenes's Sieve, being a simple method of finding the prime numbers, p. 3[illegible]7.

Hudson's bay, several animals sent from thence and described, p. [illegible]70.

Hunter, Mr. John, on the digestion of the stomach after death, p. 447.

I.

Ice-house, temperature in it moderate, p. 28[illegible].

India has the more ancient remains of arts, sciences, and civilization; p. 354, 355.

Inflammable air, extracted from most kind of substances, p. 171. Differs in smell when made of vegetable, animal, or mineral substances, p. 172. Thought to be immiscible with water, p. 173. Rendered less inflammable, and even destructive of flame, by standing long, or being strongly agitated, in water, p. 174, 180. Kills animals instantaneously, p. 175. Immiscible with fixed air, p. 175. Partly absorbed by water, p. 179. The remaining part rendered fit for respiration, and like common air, p. 180.

Jadda, a port on the Red Sea, its longitude and latitude, p. 77.

K.

Kalm, Mr. his account of a swallow found 20 degrees from the American shore, considered, p. 288.

Land-rail,

L.

Land-rail cannot fly over the ſea, p. 318.
Letters, the ancient Roman, were Etruſcan, p. 63.
Lexell, Mr. of Peterſburgh, his determination of the Sun's parallax from the obſervations of the tranſit of Venus, p. 69, &c.
Lime-kilns, uſeful in putrid diſorders, p. 205.
Lightning, effects of a violent flaſh, on the chapel at Tottenham Court Road, p. 233. Struck and killed a man there, p. 135.
Linnæus, his ſpecific characters of the rabbit conſidered, p. 6.
Lyndon, in Rutland, meteorological obſervations in that place, p. 43, 44.

M.

Malacca, a ſingular bird from thence deſcribed, p. 1, 2.
Maſkelyne, Rev. Nevil, communicates a paper of the late Dr. Bradley on the common micrometers, p. 46. His improvements of Hadley's quadrant, p. 99, &c.
Matlock water examined, p. 459.
Meteorological obſervations at Ludgvan in Cornwall, p. 365.
Mice, employed in the experiments about the noxiouſneſs of air, p. 175, 182, &c. How kept, p. 249. Live without water, p. 250.
Micrometers, the uſe of them deſcribed by Dr. Bradley, p. 46, &c.
Milky appearance of ſome ſpots of water in the Red Sea aſcribed to animalcules, p. 93, 94.
Mocha on the Red Sea, draughts of its road, p. 77. Its latitude and longitude, ibid.

Monro, Dr. Donald, his account of several mineral waters in Scotland, p. 15.

N.

Nairne, Mr. contriver of a new dipping-needle, p. 476. His experiments with it, p. 477.
Natural History, its progress during several centuries and among different people, p. 295.
Newland, Capt. Charles, observations in a voyage to the Red Sea, p. 77, 78, &c. His method of distilling fresh from sea water, p. 90. His observations on the milky appearance of some spots of water, p. 93.
Nicomachus, Extracts from his arithmetic about Eratosthenes's sieve corrected and explained, p. 339.
Nightingales, whether they can migrate at any distance, p. 300. Not attended to at certain times, p. 32.
Nitrous air, formed from a solution of metals in spirits of nitre or aqua regia, p. 210. Its reduction of common air, p. 211. The best test of the fitness of air for respiration, p. 214. Its phenomena with different kinds of noxious air, p. 215, 216. Reduced to one fourth by a mixture of iron filings and brimstone, p. 217. Noxious to plants and animals, *ibid.* Readily absorbed and obstinately retained in water, p. 218, 219. A great preserver from putrefaction, p. 223. Proportion in which it may be got from several metals, p. 322.
Numbers, See *Prime*.

O.

Oak, a new species observed and reared by Mr. Lucombe, 128. Its speedy growth described by Mr. Holwell, p. 129, &c.
Oboles, Six in a dram, p. 470.

Oenanthe crocata, a poisonous plant, found to have great virtues in the cure of some cutaneous disorders, p. 470, &c.

P.

Parallax of the Sun, deduced from the observations of the last transit of Venus, p. 69, &c.

Parallelism of the two surfaces of the index glass in Hadley's quadrant, necessary for the exactness of observations, p. 115, 116. How the errors arising from the want of it may be remedied, p. 116, 117.

Pemberton, Dr. Henry, his geometrical solutions of some astronomical problems, p. 434.

Percival, Dr. Thomas, on the waters of Buxton and Matlock, p. 455.

Person killed by lightning, p. 135.

Phlogiston, an overload of it may infect air, p. 231. and is probaby absorbed by growing plants, p. 233.

Pitkeatly, near Perth, its purging water described by Dr. Wood, p. 27. Analysed by Dr. Monro, p. 27, 28.

Plants, in a state of vegetation, prevent the alteration which flame produces in the air, p. 166. And restore it when vitiated, p. 163, 169.

Porcupine quills, dyed by the natives of Hudson's Bay in red and yellow, p. 46.

Portsmouth, its latitude deduced from astronomical observations, p. 38.

Praeneste the town of, worshiped Fortune, p. 63.

Priestley, Dr. Joseph, his observations on different kinds of air, p. 147. His description of Mr. Henley's new electrometer, p. 359.

Pultney, Dr. Richard, on the medicinal virtues of a poisonous plant, p. 469.

Prime numbers, how to be found, p. 328—332.

Ptarmigan, the same bird in Europe and in America, p. 390.

Putrefaction, see *Air*, *Vegetation*.

Pyrmont water imitated by means of fixed air incorporated in common water, p. 151, &c.

Q.

Quails, whether migratory, p. 272.

R.

Rabbit, not indigenous in Sweden, p. 6. Which of them have red pupils, ibid. Difference between a warren and a tame rabbit, p. 7. See *Hare*.
Ray, his characteristicks of the hare and rabbit examined, p. 4, 5.
Redwings, their migrations considered, p. 313, 314.
Romayne, Thomas Esq; his observations on Atmospherical electricity, p. 137.
Root, used by the Indians at Canada and at Hudson's bay to dye in red and in yellow, p. 55. Ascertained and tried by Mr. Forster, p. 56, &c.

S.

Sea Salt, the strongest spirit of, consists of two thirds of pure water, p. 239.
Snipes, constantly in some part of England, p. 306.
Solar eclipse observed in George Island, p. 34, 35.
Solway Moss, its irruption described, p. 123, 124. Phenomena attending this sudden inundation, p. 125, 126.
Sors, or Fortune, the goddess on several Denarii of the Plaetorian family, p. 61. Worshiped at Antium and Praeneste, p. 63.
Spinach, the most effectual plant in restoring vitiated air, p. 170.

Stomach cannot act upon itself during life, p. 449. But destroys itself after death, p. 450. This appearance more sensible after violent death, p. 452.
Storks never cross the sea from Holland to England, p. 319, 320.
Sun's altitude how to be observed with the quadrant, p. 128.
Swallows, whether they migrate over the sea, p. 276, 291. Different species confounded, p. 280. Found torpid and clustered together in a pond, p. 289. In the Rhine, 297. And in several other places, p. 298.
Swinton, Rev. John, an account of a Denarius of the Plætorian family, p. 60.

T.

Temperature comparative, of several waters, p. 464.
Tides, observations on them in the South Seas, p. 358.
Tisavoyanne jaune, what root it is, p. 54.
Tropic found, p. 445.
Tully, a passage of that author relative to the deities named Sortes, explained from an ancient coin, p. 62.

V.

Vapour of spirit of salt, p. 235. Its properties, ibid.
Vegetation restores air vitiated by flame, p. 166. And that which has been tainted by respiration or putrefaction, p. 194, &c.
Vitriolic acid, no sign of it in fixed air, p. 253.

W.

Walker, Mr. John, his account of the irruption of Solway Moss near Carlisle, p. 123.

Water,

5

Water imbibes fixed air, p. 151. And inflammable air, p. 180, 181. Absorbs in part putrid air, p. 191. Restores all kinds of noxious air, p. 200. Seems to decompose air, p. 247.

White lead, its effluvia noxious, p. 231.

Witchel, Mr. George, some of his Astronomical observations at Portsmouth, p. 33.

Woodcocks, where they breed, p. 308, 309. Sleep in the day time, p. 309. If seen in the night, mistaken for owls, p. 311.

Woods, not unhealthy, p. 200.

Z.

Zodiac, signs of the, delineated in several temples in India, p. 353. Probably had their origin from thence, p. 354.

The End of the SIXTY-SECOND VOLUME.

*** There are FOURTEEN Copper-Plates in this Volume, as Table IV. is double.

ERRATA.

Vol. LXI.

Pag. 139. line 11. *from the bottom, read* upon, with regard to
141. l. 1. *notes, erase the comma after* Ex,
143. *notes*, l. penult. *r.* Archiepiscopis. l. 15. *r.* Redleiam
144, l. 2, *r.* Dena. *Notes*, l. 14. *from the bottom*, *r.* Noewera, l. ult. *r.* Vincentii.
145. *notes*, l. 4. *r.* Crevecor.
147. l. 3. *the 4th letter in the Saxon word should be* ʒ.

Vol. LXII.

Pag.	xi.	line penult.	*for* vingtimee	*read*	vingtieme
	6.	6	Caniculus		Cuniculus
	8.	1.	male		mule
	ibid.	14.	is in other		is other
	37.	7.	Juptiter		Jupiter
	55.	21.	grows		it grows
	75.	21.	distantis		distantia
	77.	22.	(Tab. IV.)		(Tab.IV.&Tab.IV.*)
	125.	note †, l. 4.	weter		water
	146.	8,	them		it
	303.	note *, l. 2.	Aëdologue		Aëdologie
	314.	17.	cough		chough
	388.	21.	Three-toxd		Three-toed
	426.	17.	vesti		vestiti
	429.	6.	mandibu		mandibula
	457.	27.	property		proportion
	462.	note, line last,	207		287.

Date of issue.	Date of issue.	Date of issue.